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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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*The* Journal *of*
# Parapsychology

Volume 18 DECEMBER, 1954 Number 4

## A PK EXPERIMENT WITH THIRTY DICE RELEASED FOR HIGH- AND LOW-FACE TARGETS

By G. L. MANGAN

**ABSTRACT: The experiment reported here was carried out to test the hypothesis of PK, primarily for the personal satisfaction of the experimenter. Dr. Mangan was his own subject. He tried to influence the fall of 30 dice at a time, mechanically released, to turn up either high dice (4's, 5's, 6's) or low dice (1's, 2's, 3's), whichever the target might be. In the first half of the experiment, G. L. M. worked alone. In the second half, another member of the laboratory staff was present as witness and recorder. The results on 12,000 single die-throws were significantly above the average expected by chance, with a P-value of .000,000,2. The data were examined for position effects within the column and in the set, and a suggestive incline was found.**

**Dr. Mangan, the author, has recently become a member of the Parapsychology Laboratory of Duke University. This is his first published report in the field of parapsychology.—Ed.**

### METHODOLOGY

THE EXPERIMENT reported in this paper was carried out in the summer of 1954 in the Parapsychology Laboratory, Duke University, during the period from September 2 to 10. Esther Bond Foster, of the Laboratory staff, collaborated as witness and recorder in the second section of the experiment.

Before the experiment proper was begun, the subject, G.L.M., completed a series of 100 releases of 30 dice each for the six target faces in rotation. There was no appreciable difference between the number of hits for the various targets, but an analysis of the distribution of faces other than the target on each release indicated that the subject was scoring higher on the 4's, 5's, and 6's than on the

1's, 2's, and 3's. Accordingly the main experiment was set up with the targets separated into high dice and low dice. It was predicted that the scores on high dice would be higher than those on low dice.

A set of 30 small-sized dice measuring 3/8 in. along the edge were used throughout the series. They were commercial dice of plastic material, with spots set in excavated holes.

The dice were mechanically released. They were placed randomly in an inclined chute. On release by a press-button, they rolled down a 22-inch slope, angled at approximately 45 degrees, into a padded box, of dimensions 22 in. by 27 in.

There were 400 releases of the 30 dice, or 12,000 single die-throws. In 200 releases, the target was the high faces (4's, 5's, and 6's); and in the remaining 200, the target was the low faces (1's, 2's, and 3's). The whole series was divided into two sections, A and B, each consisting of two sets of 100 releases; a set contained four sessions of 25 releases, each session being completed in a single sitting. In chronological sequence, the first hundred releases were for high dice, the second hundred for low; these constituted Section A. The third hundred releases were for high dice, the fourth hundred for low; these comprised Section B.

G.L.M. was the subject executing the first section (eight sessions, 200 releases); no other person was present during this part of the experiment. The results, therefore, are entirely unwitnessed. The target for the first four sessions was high dice; for the second four sessions, low dice. After each release, all the faces were counted as the dice lay, and the hits were recorded in columns of six entries across (one for each face) and 25 entries down. The dice were then separated into the six face groups, and the record was checked.

In the second section, E.B.F. recorded. Though her function was primarily that of recorder and witness, she released alternately with G.L.M. Her series was not part of the experiment, but it seemed to introduce a competitive spirit, which G.L.M. found stimulating. The recording in this section was the same as in the previous one. In the first half of this section the separation into face groups was generally witnessed by E.B.F.; in the second half, it was entirely witnessed by her.

When 30 dice are released, the expectation is that, on the average, 15 hits per release will occur for the specified target of high or low faces.

Any deviation due to dice bias was eliminated by having the subject make an equal number of releases for high and low aim. Thus, any favoring of the target faces in one set would be canceled out by an equal disfavoring in the other set.

## Results

### *General Results*

The total of the whole experiment was 12,000 die-throws, or 500 runs in terms of the standard method of counting 24 die-throws per run. There were 6,287 hits or coincidences between the dice released and the target face. Expectation was 12 hits per run, or 6,000 for the whole series. This leaves a positive deviation of 287 and an average score of 12.57. The standard deviation for the whole series is 54.77, and the critical ratio is 5.24, which is highly significant. The odds against a score this high or higher as a chance occurrence are five million to one.

Section A gave 3,117 hits, against chance expectation of 3,000, a positive deviation of 117. The standard deviation is 38.73, and the CR is 3.02, which is significant. In Section B, there was a positive deviation of 170, with a corresponding CR of 4.39 and a probability of .00001. The total results are shown in Table 1.

Table 1

General Results

| | Target | No. of Runs | Average | Total Dev. | SD | CR |
|---|---|---|---|---|---|---|
| Set 1..... | High Dice | 125 | 13.02 | +128 | 27.39 | [4.67] |
| Set 2..... | Low Dice | 125 | 11.91 | − 11 | 27.39 | insig. |
| Total Section A..... | | 250 | 12.47 | +117 | 38.73 | 3.02 |
| Set 3..... | High Dice | 125 | 12.91 | +114 | 27.39 | [4.16] |
| Set 4..... | Low Dice | 125 | 12.45 | + 56 | 27.39 | [2.04] |
| Total Section B..... | | 250 | 12.68 | +170 | 38.73 | 4.39 |
| Total Series..... | | 500 | 12.57 | +287 | 54.77 | 5.24 |

The SD's given above have been calculated from the formula $\sqrt{npq}$ where $n$ = total number of dice released, $p$ = probability of success of each release, and $q$ = probability of failure of each release. It may be that the dice are biased to give a higher probability of

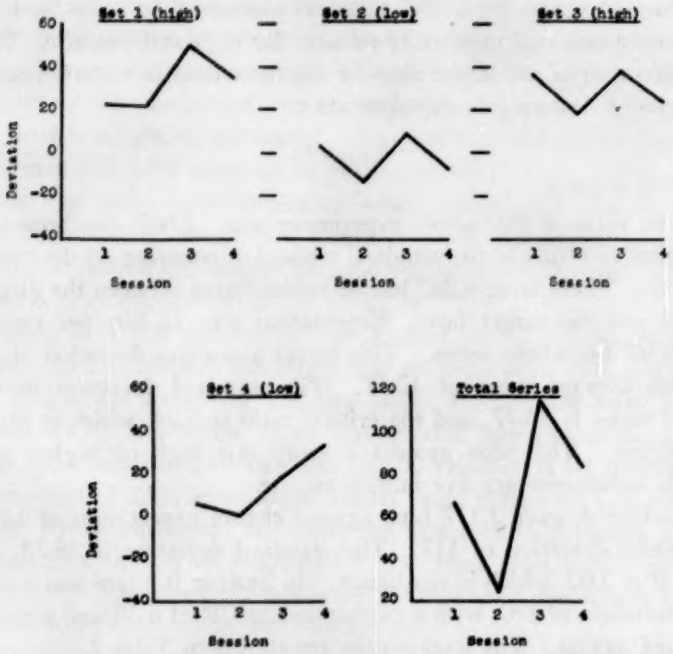


FIG. 1. Chronological distribution of deviations within sets and for all sets pooled.

success for high faces. However, as the editor's note at the end of the paper shows, the actual SD cannot exceed this value whatever the bias of the dice.

*Position Effects*

The data of the entire series were subjected to an examination of the hit distributions.

Analyses for position effects have been based generally on the record page. In this research, however, not all the records were kept with due regard to formal page structure. In the first section, two sessions of 25 releases were recorded on each page, and each session was completed at a different time. In the second section, however, when E.B.F. was releasing alternately with G.L.M., only one session was recorded on each page. Any partition of the page into equal parts thus proved impracticable.

The only plausible means of investigating position effects seemed

to be through chronological distribution, as shown by hits in the four sessions within the sets, and through vertical distribution, as evidenced by hits in the several segments within the sessions.

*Chronological Distribution in the Set.* Chronological decline has been an almost invariable accompaniment of the analyses of PK test results (1, 2). In this study there was one column of 25 entries for each session of the set. The results for the whole experiment are shown in Table 2, in terms of deviations or number of hits above expectation for each of the four sessions in each set. These results are graphically represented in Figure 1.

Table 2

CHRONOLOGICAL DISTRIBUTION OF DEVIATIONS IN THE SETS

| Set | Target | Session | | | |
|---|---|---|---|---|---|
| | | 1 | 2 | 3 | 4 |
| 1 | High Dice | +22 | +21 | +50 | +35 |
| 2 | Low Dice | + 3 | −14 | + 8 | − 8 |
| 3 | High Dice | +36 | +18 | +37 | +23 |
| 4 | Low Dice | + 5 | 0 | +18 | +33 |
| Total | High Dice | +58 | +39 | +87 | +58 |
| Total | Low Dice | + 8 | −14 | +26 | +25 |
| Total Series | | +66 | +25 | +113 | +83 |

$CR_d$ (1, 2−3, 4) = 1.92; P = .055

The contrast between the first and second half of the set (that is, sessions 1 and 2 *versus* sessions 3 and 4) gives a CR of the difference of 1.92, with a probability of .055. It is worthy of note that there is an incline rather than the usual decline. The deviation of +33 for the last session of low aim is interesting—the CR is 2.41—but it is not significantly different from the combined other sessions of low aim.

*Vertical Distribution.* It has been reported on numerous occasions that the first and last segments of the column gave very much higher deviations than did the middle segments (3, 4). In this research each column (comprising 25 releases) was divided into five segments, and the distributions of hits in each segment were compared. In this way cross-sections of distributions of successes

for the entire results are shown. These are set out in Table 3 in terms of the deviation for each segment, and are graphically represented in Figure 2.

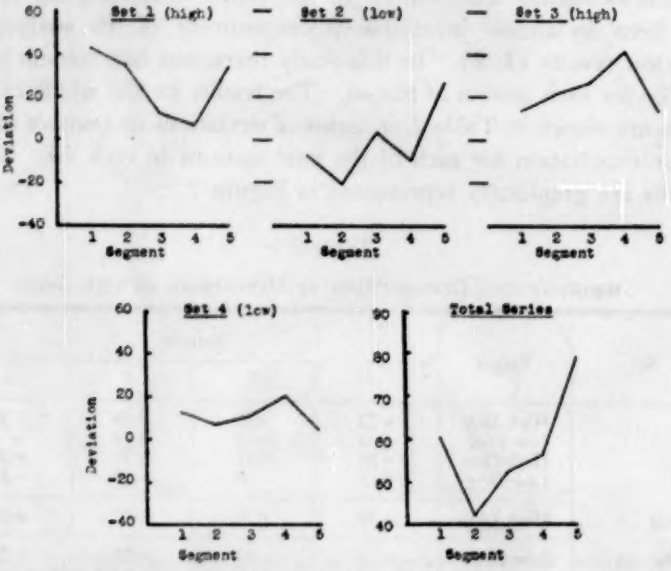


FIG. 2. Vertical distribution of deviations by segments in columns (sessions) in each set, and for all sets pooled.

Table 3

VERTICAL DISTRIBUTION OF DEVIATIONS AMONG THE SEGMENTS IN THE COLUMN (SESSION)

| Segment | Section A | | Section B | | Total |
|---|---|---|---|---|---|
| | Set 1 | Set 2 | Set 3 | Set 4 | |
| 1..... | +42 | − 8 | +13 | +13 | +60 |
| 2..... | +35 | −21 | +21 | + 7 | +42 |
| 3..... | +12 | + 2 | +27 | +11 | +52 |
| 4..... | + 6 | −10 | +40 | +20 | +56 |
| 5..... | +33 | +26 | +13 | + 5 | +77 |

In the totals of the five-release segments in the 25-release sessions, there is a slight difference only in the first four segments: +60, +42, +52, and +56 were the segment deviations. The final segment, with a deviation of +77, displays some terminal salience.

The typical U-curve of the vertical distribution has been reported often for ESP results, but decline has been the usual thing in PK. The finding here, though not in itself significant, when combined with the chronological incline, gives at least a suggestive effect of incline where decline has generally been found. It may be, however, that the shortness of this series compared with most has in some way contributed to this incline effect.

## Discussion

In view of the conditions of the experiment, under which equal numbers of releases were made for high and low faces, the results cannot in any way or degree be accounted for by the hypothesis of faulty dice structure. Any faults the dice may have had would have been equalized in their effects by the technique of releasing alternately for high and low face. The use of skill was guarded against by the mechanical release of the 30 dice at a time.

The only factor which might vitiate the results is that of recording error. Usually it is possible to get at this problem in a conclusive way by the analyses of position effects (5). The analysis of relative success according to position in the throw sequence of the test should show no particular distribution of success because of recording or other errors. Recording errors cannot be considered to be localized to this extent; rather, such position effects would be attributable to the PK effect itself.

Although the position effects in this analysis are not striking enough in themselves to counteract the hypothesis of recording errors, there are suggestions that the error hypothesis is highly unlikely. The separation of the dice into face groups, the counting and recording as the dice lay, the rechecking of the dice in their groups, and the rechecking of the records appeared to ensure an acceptable level of accuracy. In the second section particularly, where the separation, counting, and recording were usually checked by a witness after each release, the possibility of recording error is quite small. It is interesting to note that the deviations in the second section are appreciably greater than those of the first.

At this stage of PK research, the psychological factors are of special interest (6). It was necessary, in this experiment, to face the possibility that the subject might have a preference for high dice and that the dice might be biased in favor of the high faces so

that the two factors in combination might have contributed to the scoring results. Hence it seemed important to try to estimate, as nearly as possible, where the empirical mean lay, and thus determine the true high- and low-face deviations.

An attempt was made to obtain a control series. One of the Laboratory members, Winifred Nielsen, was asked to be the subject. In order that she might have as unbiased an attitude toward her task as possible, the experimenter resorted to a subterfuge. He asked her to complete a "random" series with 12 colored dice. At no stage was she able to see the dice or the release mechanism. After two releases of the colored dice, the 30 experimental dice were substituted, without her knowledge. The subterfuge was maintained throughout the series of 100 releases.

From these 100 releases the deviations for each face were as follows:

| 1-face | 2-face | 3-face | 4-face | 5-face | 6-face |
|---|---|---|---|---|---|
| −22 | −7 | −2 | +6 | +3 | +22 |

The empirical means from these data are 1,531 for high dice and 1,469 for low dice for the 3,000 single die-throws. The corrected deviations in the four experimental sets are:

| | A | B |
|---|---|---|
| High | +97 | +83 |
| Low | +20 | +87 |

as compared with the uncorrected figures:

| | A | B |
|---|---|---|
| High | +128 | +114 |
| Low | − 11 | + 56 |

The uncorrected deviations are undoubtedly a composite of true deviation, physical dice bias, which favors the high faces, and the

subject's preference for high faces. And since the subject in the "random" series initially had no knowledge that the experimental dice had been substituted for the colored dice, a condition which precludes the factor of face preference, it is probable that the deviations in the control series are a reasonably accurate estimate of physical dice bias. There is only a remote possibility that the subject in the control series arrived at a knowledge that the substitution had been made and that she used this information to exert some influence on the fall of the dice. That is, it is unlikely that a preference could have been operating.

The corrected figures are probably a more accurate estimate of the true deviations. It is interesting to note that in Section A the subject obtained a large deviation on high dice and a low deviation on low dice, a result that was predicted before the start of the experiment. In the second section, however, the subject concentrated on obtaining a significant deviation on low dice. In this he was successful (in terms of the corrected figures), while maintaining the significant deviation on the high faces.

The positive deviations obtained in this series of releases for high and low dice can be placed at a very high level of significance. The most likely explanation of the results is PK. The secondary analyses of the hit distributions evidenced some lawfulness in its operation.

Editor's Note: Maximum variance for die throws.

Suppose there are $n$ independent binomial trials under each of the probabilities $p_1, \ldots\ldots p_k$, and $p_1 + \ldots\ldots + p_k = 1$.

In order to maximize $\sigma^2 = n(p_1q_1 + \ldots + p_kq_k)$ using Lagrange multipliers, one forms

$$z = n(p_1q_1 + \ldots\ldots + p_kq_k) + \lambda\ (p_1 + \ldots\ldots + p_k - 1)$$

and sets $\dfrac{\partial z}{\partial p_i} = 0,\ i = 1, 2, \ldots\ldots, k.$

This gives $p_i = \tfrac{1}{2}\ (1 + \dfrac{\lambda}{n})$.

Then $\sum_1^k p_i = 1 = \dfrac{k}{2}(1 + \dfrac{\lambda}{n})$ or $\dfrac{\lambda}{n} = \dfrac{2}{k} - 1.$

Substituting in $p_i$, one obtains $p_i = \dfrac{1}{k}$, for all $i$. Thus the variance is a maximum (it is obviously not a minimum) when all $p_i$ are equal.

In the Mangan experiment each face of each die was the target an equal number of times. Therefore, using $p_1 = \frac{1}{2}$ as the probability of a low on each die, and $p_2 = \frac{1}{2}$ as the probability of a high on each die cannot give a theoretical variance smaller than the actual variance, regardless of how the dice were biased.

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# AN APPROACH TO INSTRUMENTAL INVESTIGATION OF PSYCHOKINESIS

By H. Forwald

**ABSTRACT: The following experiment introduces a new method of evaluating the PK effect in placement tests with dice. In his former series, already reported, Mr. Forwald obtained significant results with electrically released dice which rolled down an incline onto a dice table while he, the subject, tried to influence them to come to rest on whichever side of a central dividing line had been selected as the target side of the table.**

**This method made it possible only to make a statistical evaluation of PK based on the numbers of dice on each side, but not to measure the physical force required to place them there. In the two new series reported here, it was Mr. Forwald's purpose to provide an estimate of this force. Consequently, the apparatus was adapted so that the die positions in terms of distance from the center line could be measured. By determining how far to the left or right of the dividing line each die fell, the magnitude of the forces involved could be evaluated. The method used was to mark off the dice table with lines parallel to the sides. These lines were one centimeter apart and they were numbered 0-80 from the righthand side (A) to the lefthand side (B). After each release of six cubes on each trial, the position of each cube was recorded to the nearest centimeter line.**

**In 1260 die throws, comprising two series, significant PK effects were attained, the greater part of which could be accounted for by the first throws for one (the A-) side of the table. From the placement readings, an estimate of the side force required to produce this amount of aberration was worked out. The mathematical method of calculation is given in the Appendix.**

**A comparison of this new method of using the measuring scale and the old method of using only a dividing line showed the new method to be the more sensitive of the two as applied to the data of these two series.—Ed.**

## Introduction

In previous reports (1, 2, 3, 4) experiments in psychokinesis are described, in which electrically released cubes rolled down an incline and scattered over a horizontal dice table which was divided longitudinally into two equal areas. Attempts were made to "influence" the cubes to come to rest in the area specified as target. In order to eliminate the effect of bias in the arrangement, each of the areas was used an equal number of times as target. As a rule the target was alternated after ten releases.

In 71,500 die-throws carried out with conscious attempts to influence the dice there was an excess in hits on the target side over that expected by chance of 691, which corresponds to 5.17 standard

deviations. So large a deviation would occur by chance in only one out of three million such experimental series.

The conclusion which can be drawn from this result is that the cubes have been subjected to a side force of an unknown kind, which has caused an aberration in their natural direction of movement, and that this force has acted on the throwing area in the direction that would be necessary to place a significant number of the cubes according to the subjects' desire. The immediate impression is that a mere psychological state has been responsible for a physical effect on free moving bodies, or that energies which may have existed in the experimental situation have been mentally controlled so as to produce suitably directed forces on the bodies.

As results of the kind set forth are strange to science, there was a need for a closer study of the forces, but for this purpose the original experimental procedure was not well suited because it allowed for dealing with the results mainly on the basis of statistics. It was, for instance, not possible to get an estimate of the magnitude of the forces involved. The experimental arrangement, therefore, was modified so that measurements could be taken of the die positions. In this way the results could—in connection with speed or running time measurements—be subjected to dynamical calculations.

It is an object of the present report to give the fundamentals of this dynamical method of investigation, and to show its application to experimental series in which the die positions were individually measured.

## Apparatus and Procedure

At first some words about the apparatus: It has an electrically released V-shaped dice container perched at the top of a sloping runway. When a button at the end of a cord is pressed the container tilts, and a row of dice—usually six in number—glides out and down an incline which curves out onto a horizontal dice table. The table was previously divided into two halves by means of a metal string spanned close to the surface. This division has now been abandoned and replaced by a coordinate system, with ink lines drawn for each centimeter in longitudinal as well as in transversal direction ($X$- and $Y$-directions respectively). Readings of the dice positions thus can be taken directly on the plane after each release.

The table measures 44 in. (112 cm.) in longitudinal direction and 31.5 in. (80 cm.) in transversal direction. Zero for the *X*-direction is at the foot of the incline, and for the *Y*-direction at the extreme right-hand side relative to the direction of movement. The right-hand side is designated by *A*, and the left-hand side by *B* (See Fig. 1.)

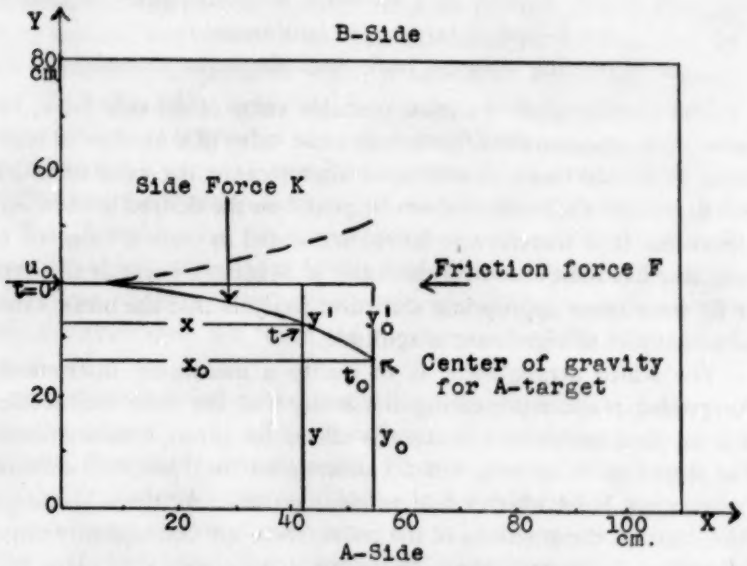


FIG. 1. Calculation of the side force K.

On the basis of the readings of the dice positions it is possible to calculate (1) the arithmetical mean value $x_o$ for the running direction; and (2) the mean value $y_o$ for the direction transverse to that of running, for a large number of falls. Physically this means the determination of the center of gravity of the cube aggregate. If two aggregates of the same number of falls have been "influenced" to opposite sides in their movement, it is to be expected, according to the previous experiences, that there will be a distance between their centers of gravity, especially in the *Y*-direction. Assuming the aggregate to be a point mass, it is possible to calculate the side force *K* which is necessary to cause the observed aberration $y_o'$; that is, half the distance in the *Y*-direction between the centers of gravity.

This calculation is shown in the Appendix, but the final formula for $K$ will be given here:

$$K = 4m \frac{y_o'}{t_o^2}$$

where $K$ = side force (in dynes)
$m$ = mass of dice for one side as target (in grams)
$y_o'$ = half distance in $Y$-direction between centers of gravity for $B$- and $A$-*targets* (in centimeters)
$t_o$ = running time on horizontal plane (in seconds)

The formula gives the most probable value of the side force, because $y_o'$ is based on the arithmetical mean value of a number of readings. $K$ should be given with a certain tolerance, the value of which will depend on the number of readings and on the desired level of significance. It is therefore to be recommended to state a value of $K$ only if it has been found on the basis of Student's $t$ test of the data or by some other appropriate statistical analysis that the mean value $y_o'$ actually is of significant magnitude.

The scaled arrangement is in reality a measuring instrument, the graded plane representing the scale, and the cube the needle. It is an ideal instrument because it allows for direct measurements. The forces to be investigated act directly on the "needle," without intermediate links which could produce errors. As there are large differences in the positions of the cubes, there are consequently large differences in the readings. Therefore it is necessary to carry out many measurements in order to get as closely as possible to the real arithmetical mean value.

When reading the die position there is no reason to proceed in a different way than usual when taking a reading on an instrument; that is, to take the reading to the nearest division line on the scale. In some cases it will be difficult to decide, and it is estimated that such a difficulty would represent at most those cases in which the center of the object was not more than 1 mm. from the mid-point between two lines on the scale. All such difficult cases were recorded so as to disfavor the target. Inasmuch as the results were recorded to the closest centimeter, the error in the total result due to such inaccuracies in the readings is negligible in an experiment comprising many hundreds of trials. Observation has shown that the reading error will not exceed one millimeter on an average, whereas

the difference between the centers of gravity for opposite targets in these placement series has given values of several *centi*meters.

The readings are taken to refer to the middle point of the cube. Such a reading is easy to make by comparing the distances of the cube edges from adjacent dividing lines.

Some doubts have recently been expressed as to whether the subject can exert any normal physical influence on the cubes when they are in motion, and whether the aberrations could be explained by such influences. Expressions like "proximity of the experimenter" and "disturbing vibrations" have been mentioned. Such expressions do not indicate exactly what kind of physical influence the questioner has in mind. Under the actual circumstances only mechanical and aerodynamical influences seem to be worth consideration.

That no mechanical vibrations of consequence can be transferred from the subject to the cubes is seen from the fact that the apparatus stands on a concrete floor, which rests on a solid foundation. The subject further does not touch the apparatus when the cubes are in motion.

Aerodynamical influences would have to consist of air streams from the subject. The effect of such streams on the moving cubes has been thoroughly investigated. Thus it was calculated that an air velocity of about two meters per second is required in the middle of the table to account for the observed total aberration in the first series. It was found to be impossible to produce such velocities from the place of the subject, even if he was intentionally blowing from his mouth in the direction of the table. The fact is that the subject stands calm and motionless, and with his mouth closed when the dice are rolling. It may also be pointed out that the largest aberrations (which would require an air velocity of about four meters per second in the middle of the table) have been observed *in the direction of* the subject, and not *away from* him, which decidedly argues against any explanation on the basis of air streams.

The scaled tests dealt with in this report comprise the following series:

S1 (placement series with 10 sets, the first one comprising 10 releases for *A* followed by 10 for *B*, and the remaining 9 sets having 5 releases for *A* followed by 5 for *B*).

S2 (control series with 10 sets of 10 releases without target).

S3 (placement series with 10 sets of 5 releases for *A* followed by 5 for *B*).

In S1 and S2, 12 cubes per release were used in Set 1, and six in the following sets. In S3, six cubes per release were used in all sets. The cubes have a side of 16 mm. and are of wood. S1 and S2 comprise 660 trials each; and S3 comprises 600 trials.

Readings were taken visually in S1 and S2. In S3 the cube distributions were photographed after each release, and readings were taken on the pictures by two independent investigators at Duke. Complete readings were taken also by the author, from the films. There were only small mutual differences in the three series of readings. My own readings were somewhat on the conservative side relative to the Duke readings, the difference between averages for *B*- and *A*-targets being 1 percent smaller in my case.

I was my own subject throughout the entire experiment.

I stood in all series on the *A*-side of the apparatus, so that an aberration of the cubes to the specified side would involve a "pulling" effect when *A* was the target and a "pushing" effect when *B* was the target. The placement series were carried out with only one set per session, and there was generally a time interval of one or more days between the sessions. The control series was carried out on one day.

## Results

Table 1 contains the results from a Student's *t* test of the difference between the readings for *A*- and *B*-targets in the two placement series. It gives for the visually recorded series, S1, a P of .001, and for the photographed series, S3, a P of .08. When both series are combined and *t* is computed for the total results considered as a single series, P is .0003. The difference between the average reading on the 80 cm. scale when throwing for *A* and for *B* is in the direction that is consistent with the subject's intention. In Series S1 this difference amounts to 4.23 cm., and in Series S3, to 2.21 cm. Assuming the same sequence of target sides in the control series (S2) as in the placement series, the control series gives a difference of .08 cm. between the averages, with a minus sign of the difference as compared with the placement series. The small difference in the control series is entirely at the chance level.

These summary data do not give a complete picture of the results.

While carrying out the experiments, I could not avoid observing that the effect on the cubes was most pronounced in the first release in the set for *A* as target, and the subsequent analysis confirmed this observation and showed that there was a significant difference in placement effect among the five throws for the *A*-side.

The total frequency with which cubes came to rest on the surface within the ranges 0-20, 21-40, and 41-80 in the *Y*-direction when the *A*-side was target was tabulated separately for each of the five throws of the pooled half-set. A chi-square test applied to this three-by-five matrix gave $\chi^2 = 20.42$; 8 d.f.; $P = .01$. It was clear from inspection of the table that the unusually large number of dice stopping within the 0-20 area on the first throw accounted for the significant value of chi square. A similar test applied to the pooled half-set with the *B*-side as target yielded only chance results, indicating only random variation in success among those throws. The significance of the placement series is in fact mainly due to the outstanding results in the first throw in the set.

TABLE 1

EVALUATION OF SCALED PLACEMENT RESULTS

| Series | A-TARGET | | B-TARGET | | $t_d$ | P |
|---|---|---|---|---|---|---|
| | N | Average Position (in cm.) | N | Average Position (in cm.) | | |
| S1..... | 330 | 36.41 | 330 | 40.64 | 3.28 | .001 |
| S3..... | 300 | 36.10 | 300 | 38.31 | 1.75 | .08 |
| S1+S3..... | 630 | 36.26 | 630 | 39.53 | 3.61 | .0003 |
| S2 (Control) | 330 | 38.93 | 330 | 38.85 | .... | .... |

As this circumstance could give rise to suspicion that the mechanical conditions of the apparatus were different in the first release, a thorough investigation of the behavior of the mechanism was undertaken. The point being a vital one, it is necessary to deal with it at some length.

The dice release mechanism consists of a trapdoor-like platform which turns in the vertical plane around a pivot and which has its free end over the incline down which the cubes roll. It has a V-shaped channel with a 90-degree angle for the cubes, which are thus exactly located relative to the trap. The V-channel is parallel

to the center line of the table and is located vertically over the extension of the center line at the top of the incline. The pivot allows a very limited play to the release mechanism in the lateral direction. By extrapolating the play to the middle of the table it is found that it can only account for differences of about 1 mm. between the centers of gravity of the cubes on successive throws. The trap device is held in the starting position by means of a hook attached to the anchor of an electromagnet. The hook has a stop which assures the constancy of the starting position. The magnet works very fast and independently of the time of contact in the push-button. When released, the trap swings down, due to gravity, and is stopped by a damping cushion after having passed through an angle of about 45 degrees from the horizontal position. After a fall in free air the dice hit the incline and scatter over the dice table.

The only mechanical circumstance which could conceivably have produced the results obtained is a sort of sticking of the mechanism after a period of disuse in such a way that it favored the *A*-side on the first release. During the investigation, however, no sticking tendencies could be discovered. A mere slowing down in the release could not have the observed effect because it would not change the direction of the channel in the lateral direction. It can also be mentioned that the trap release was always left in the "down" position after a set was finished. It therefore had to be lifted to the starting position and the magnet hooked in just before the first release in the set. This would automatically break up any sticking resulting from a period of disuse.

To be quite sure that the conditions were not different in the first release a series of releases was carried out with about the same timing as in the placement series, but without any attempt to influence the dice mentally. For this purpose, one release was made per day. The results of this series were entirely at the chance level. The cubes showed no tendency to favor the *A*-side (an average of 39.57 cm. was arrived at in 60 trials).

As a result of this investigation I was forced to the conclusion that the pronounced aberrations in the first throw for *A* were not due to any fault in the set-up, but that they must be attributed to a physical influence of an unknown kind.

In view of the results showing the significance of the total data

and the $\chi^2$ test showing that the decline effect in the set was significant due to the fact that the first throw was so outstanding, it was interesting to evaluate the results in Throw 1 separately as the point at which the PK effect was concentrated. A Student's $t$ test was therefore applied to the difference between the first throws for the $A$- and $B$-targets, and the results are given in Table 2. It is seen from the table that the effect in Throw 1 is highly significant in each of the placement series. For the two series combined the $t$-value is 5.71 with a corresponding odds-to-one against chance of more than ten million.

TABLE 2

RESULTS FOR FIRST THROW IN THE FIVE-THROW UNIT

| Series | A-TARGET | | B-TARGET | | $t_d$ | P |
|---|---|---|---|---|---|---|
| | N | Average Position (in cm.) | N | Average Position (in cm.) | | |
| S1..... | 66 | 30.11 | 66 | 42.67 | 4.20 | .00003 |
| S3..... | 60 | 26.72 | 60 | 37.72 | 3.89 | .0001 |
| S1+S3..... | 126 | 28.49 | 126 | 40.31 | 5.71 | <.0000001 |
| S2 (Control) | 66 | 42.71 | 66 | 37.88 | 1.76 | .078 |

In the control series, S2, the dice favored the $B$-side in the first release in the set, but to an insignificant degree, the probability being .078.

A comparison of the $t$-test results for Series S1 in Table 1 with the results when the data are evaluated by the method used heretofore (involving the distribution of hits and misses relative to a dividing middle line) suggests that measuring PK by the new scaled method yields a higher order of significance in a given number of trials than by the previous method; that is, it is more sensitive. The hits on the target side of the grand average for the whole series (38.53 cm.), give a deviation of +17 hits above the chance value of 330. This deviation has a CR of 1.33, and a P of .18 by the old method, as compared with a P of .001 with the more accurate new method.

It is interesting to note that *each* of the ten sets in Series S1 gave a positive difference; that is, a difference in the expected direction, between the $y_0$-values of the centers of gravity for $B$- and

*A*-targets. This would occur by chance in only one out of $2^{10} = 1024$ such series. The differences in the ten sets were: 2.82, 4.67, 3.50, 4.06, 7.53, 4.53, 2.30, 2.03, 5.10, and 7.10 cm., with an average of 4.23 cm. per set of 60 trials.

To ordinary common sense a straight-on result like this would give full certainty that a specific factor was at work. The test conditions exclude any normal physical explanation. There are with six cubes released simultaneously, $6^6 \cdot 6! = 33{,}592{,}320$ different initial positions in which the dice aggregate can be placed in the container. This circumstance alone gives a wide field for randomization, because the dice are always brought to start in complete disorder (they are blank cubes without marking on their sides). Also, the variable friction conditions in the channel during the release have a randomizing effect; and as special investigations have shown, it is absolutely impossible for the experimenter to have any opinion about how the friction conditions in the channel influence the results.

## Calculation of Side Force

As it has now been shown that the results are strongly indicative of an effect besides chance, we are entitled to carry out a calculation of the side force necessary to cause the observed aberration of the cubes from their natural direction of movement. It seems justified to consider only the first release in the set, because only in this release is there full assurance that extraphysical forces have been at work.

The aberration $y_o'$ in the lateral direction in the first release may be taken as the difference between the average reading for the whole series and the average in the first release for *A*. Thus:

In S1: $y_o' = 38.53 - 30.11 = 8.42$ cm. (in 66 die-throws).
In S3: $y_o' = 37.21 - 26.72 = 10.49$ cm. (in 60 die-throws).
Total: $y_o' = 37.91 - 28.50 = 9.41$ cm. (in 126 die-throws).

These values are on the conservative side because the averages for the first throw are included in calculating the averages for the entire series. The average readings for the whole series would have been greater (the centers of gravity nearer to the *B*-side) if the aberration in the first release had been of the same magnitude as in the remaining releases in the set instead of being greater, as was actually the case.

It is convenient to calculate the side force acting on an average on a single cube. The weight of one cube is 2 grams. Measurements of the running time $t_o$ have given an average of about .5 seconds. The formula for K gives, with these values, the following average side forces per cube in the first release in the set:

In S1: K = 270 dynes, or 13.8 percent of the cube weight.
In S3: K = 335 dynes, or 17.1 percent of the cube weight.
Total: K = 300 dynes, or 15.3 percent of the cube weight.

These values are calculated under the assumption that all six cubes in the release were acted on by the force during their entire movement. To judge from observations during the experiments, it is not probable that this was the case. If, for instance, only three cubes, on the average, were acted on, then the force per influenced die would be twice the calculated values. For this reason the values calculated above should be considered as minimum values.

The aggregate forces which have acted on an average in each release of the six cubes are for Series S1, 1615 dynes; for Series S3, 2000 dynes; and for both series, 1800 dynes.

## Discussion

Advantages of the new "scaled placement method" dealt with in this report seem to be the enlarged sensitivity as compared with the previous two-area method—enabling the use of relatively short series—and the possibility it offers for investigating energetical aspects. The described experiments represent samples designed for the purpose of getting an estimate of the magnitude of the side force when a lateral aberration of the movement is specified. Naturally the design may be varied in order to study different aspects. Records may for instance be taken in both directions, $X$ and $Y$, for a closer investigation of the direction of the forces. As there are indications that the forces are dependent on the location of the experimenter, a further inquiry on this point seems promising.

The calculated average side force equivalent to a weight of about 2 grams, acting on the aggregate of 6 wooden cubes weighing 12 grams in total, is so large that it apparently cannot be accounted for by any known agency, at least not one of a generally recognized type. The conditions under which the effect occurred, together with the circumstance that the direction of the force corresponded with the

experimenter's intention, justify calling the effect a psycho-physical one; and it is convenient to refer to it by the previously introduced designation PK (psychokinesis).

A further discussion of the energetical problem seems not to be useful at present because the experimental material is still rather limited. But new series are under way, and it is hoped that further material can be presented in the near future.

## Appendix

The calculation of the side force $K$, which is necessary to produce an aberration $y'$ of the cube aggregate from its natural straight line movement involves the assumption that the force $K$ acts with a constant rate in the $Y$-direction during the movement on the horizontal plane. An alternate assumption would be that of a momentary force acting on each die at a strategical instant or a series of such instants. However, the assumption made here—that the force acts at a constant rate—is simpler from the point of view of calculating the value of $K$, and it is consistent with the present purpose of getting a general (and conservative) estimate of the magnitude of the force.

It is further assumed that a friction force $F$ of a constant rate acts in the direction opposite to that of movement. It has a component in the $X$-direction, $F_x$, and a component in the $Y$-direction, $F_y$, both $F_x$ and $F_y$ being variable during the travel. The relation between the friction forces is $F^2 = F_x^2 + F_y^2$. Due to the aberration $y'$ being small as compared with the traveling distance $X$, the component $F_x$ will differ very little from $F$, and the calculation will give results of sufficient accuracy when assuming $F_x = F$. The friction force in the $Y$-direction then can be assumed to vary with the aberration gradient according to $F_y = F \cdot dy/dx$.* $F$ represents

* It has been suggested by one of the statistical editors that this assumption can be strictly justified only when the aberration gradient $dy/dx$ is small. Yet equations (4) and (14) indicate that $dy/dx = (K/F) \log_e t_o/(t_o - t)$, which tends to infinity as $t$ approaches $t_o$. It is recognized that the mathematical equations would be extremely difficult to solve without this assumption, and it appears that large values of $dy/dx$ occur only near the end of the trajectory of the cube aggregate, where the distance traveled is small, and may not have much effect on the result. Thus the assumption stated may well be sufficiently accurate for the purpose of obtaining a general idea of the magnitude of the force involved. A somewhat cursory analysis suggests that this assumption exaggerates the frictional force to be overcome in the $Y$-direction in relation to that in the $X$-direction and thus leads to a slight overestimate of the side force $K$.—Ed.

the average friction of the whole aggregate, and it is expressed in the calculation through the *observed* values of travel time and aggregate weight.

The traveling time $t$ is counted from the moment when the cubes enter the horizontal plane. The cubes come to rest after the time $t_o$. It is convenient to calculate an equation for $x$ as a function of $t$, and one for $y'$ as a function of $t$. Then these two equations may be combined in order to get an equation for $y'$ as a function of $x$. By inserting in this last equation the ultimate coordinates $y_o'$ and $x_o$ we will arrive at a formula for the side force $K$.

If $a_x$ is the acceleration rate in the $X$-direction, then the momentary traveling distance $x$ will be

$$x = x_o + \frac{a_x}{2}(t_o - t)^2 \tag{1}$$

where

$$a_x = -\frac{F}{m}. \tag{2}$$

$m$ is the mass of the cube aggregate for one side as target, and $a_x$ will have a minus sign because the movement is decelerated. (1) and (2) give

$$\boxed{x = x_o - \frac{F}{2m}(t_o - t)^2.} \tag{3}$$

By derivating (3) we find the momentary velocity in the $X$-direction:

$$\frac{dx}{dt} = \frac{F}{m}(t_o - t). \tag{4}$$

The momentary acceleration in the $Y$-direction is

$$\frac{d^2y}{dt^2} = \frac{K - Fy}{m} \tag{5}$$

where, as stated above,

$$Fy = F\frac{dy}{dx}. \tag{6}$$

Formulas (5) and (6) give

$$\frac{d^2y}{dt^2} = \frac{1}{m}\left[K - F\frac{dy}{dx}\right] = \left[\frac{K}{m} - \frac{F}{m}\cdot\frac{\frac{dy}{dt}}{\frac{dx}{dt}}\right]. \tag{7}$$

After introduction of (4) we find

$$\frac{d^2y}{dt^2} = \frac{K}{m} - \frac{\frac{dy}{dt}}{t_o - t}. \tag{8}$$

Substituting in (8), we have

(9) $$\frac{\frac{dy}{dt}}{t_o-t}=r \quad \text{or} \quad \frac{dy}{dt}=rt_o-rt;$$

and upon differentiating

(10) $$\frac{d^2y}{dt^2}=t_o\frac{dr}{dt}-r-t\frac{dr}{dt}.$$

Formulas (8) and (10) give

(11) $$dr=\frac{K}{m}\cdot\frac{dt}{t_o-t};$$

or, after integration,

(12) $$r=-\frac{K}{m}\log_e(t_o-t)+C.$$

Since $r$ is zero when $t$ is zero, $C=\frac{K}{m}\log_e t_o$; and

(13) $$r=\frac{K}{m}\log_e\frac{t_o}{t_o-t}.$$

Formulas (9) and (13) give

(14) $$dy=\frac{K}{m}(t_o-t)\left(\log_e\frac{t_o}{t_o-t}\right)dt.$$

Substituting in (14)

(15) $$\frac{t_o}{t_o-t}=p;\ \text{or}\ t_o-t=\frac{t_o}{p},\ \text{giving}\ dt=t_o\frac{dp}{p^2},$$

and we get

(16) $$dy=\frac{K}{m}t_o^2(\log_e p)\frac{dp}{p^3}.$$

Introducing $\log_e$, $p=u$; $du=\frac{dp}{p}$; $p^{-3}dp=dv$; and $v=-\frac{1}{2p^2}$

gives

(17) $$\int(\log_e p)\frac{dp}{p^3}=-(\log_e p)\frac{1}{2p^2}+\int\frac{dp}{2p^3}=-\frac{1}{2p^2}(\log_e p)-\frac{1}{4p^2};$$

whence

(18) $$y'=\frac{K}{m}t_o^2\left[-\frac{1}{2p^2}\log_e p-\frac{1}{4p^2}\right]+C;$$

or

(19) $$y'=-\frac{K}{2m}(t_o-t)^2\left[\log_e\frac{t_o}{t_o-t}+\frac{1}{2}\right]+C.$$

When $t$ is zero, $y'$ is zero, giving $C=\frac{K}{4m}t_o^2$. Hence

(20) $$\boxed{y'=\frac{K}{4m}t_o^2-\frac{K}{2m}(t_o-t)^2\left[\log_e\frac{t_o}{t_o-t}+\frac{1}{2}\right].}$$

In above equations (3) and (20) the moving distances $x$ and $y'$ respectively are given as functions of the time $t$. Before combining the two equations in order to get an equation for $y'$ as a function of $x$ it is convenient to eliminate $F$ and $m$ from equation (3); and this is accomplished in the following way.

The ultimate distance $x_o$ is given by

$$x_o = -\frac{a_x t_o^2}{2}. \tag{21}$$

Here we may introduce $a_x$ according to (2) with its numerical value, giving

$$x_o = \frac{F}{2m} t_o^2; \quad \text{or} \quad \frac{F}{2m} = \frac{x_o}{t_o^2}. \tag{22}$$

Formulas (3) and (22) give

$$\frac{x_o - x}{x_o} = \frac{(t_o - t)^2}{t_o}. \tag{23}$$

Formulas (20) and (23) give the following expression for $y'$ as a function of $x$:

$$y' = \frac{K}{4m} t_o^2 - \frac{K}{2m} \cdot \frac{x_o - x}{x_o} t_o^2 \left[ \log_e \sqrt{\frac{x_o}{x_o - x}} + \frac{1}{2} \right]; \tag{24}$$

or

$$y' = \frac{K}{2m} t_o^2 \left[ \frac{x}{2x_o} - \frac{x_o - x}{x_o} \log_e \sqrt{\frac{x_o}{x_o - x}} \right]. \tag{25}$$

Introduction of the coordinates $x = x_o$ and $y' = y_o'$ in (25) gives for the expression $\frac{x_o - x}{x_o} \log_e \sqrt{\frac{x_o}{x_o - x}}$ the value zero times infinity, which is undetermined. However, it can be shown graphically or by de l' Hospital's Rule that this expression becomes zero for $x = x_o$. Thus we have, for the ultimate coordinates, $x_o$ and $y_o'$,

$$y_o' = \frac{K}{4m} t_o^2; \tag{26}$$

or

$$\boxed{K = 4m \frac{y_o'}{t_o^2}}. \tag{27}$$

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# A COMPARISON OF SPHERES AND CUBES IN PLACEMENT PK TESTS

By W. E. Cox

**ABSTRACT:** In this experiment, the relative influence of placement PK upon marbles and dice was tested as they were released together onto a surface divided into two equal areas, one of which was the target on each throw. In the first of three series, the subjects concentrated upon both types of object on each release; in the second, upon dice or marbles as directed; and, in the third, upon the one or the other as they preferred. The purpose of the three series was chiefly to test the difference between dice and marbles.

In a total of 1,344 runs, there was a significant difference between the numbers of dice-hits and marble-hits, the dice scoring below chance expectation and the marbles above throughout the three series. The data lend some support to a suggestion arising from earlier studies: that a dual-aspect psi task may lead to positive scoring on one aspect and negative scoring on the other.—Ed.

THE RECENT REPORTS presenting evidence that PK can influence the place at which moving objects come to rest raise anew the question of whether the shape of the objects affects the results. In an effort to throw further light upon this question, an experiment was conducted in which 24 dice and 24 marbles were released simultaneously on each trial in a PK placement test. The purpose was to analyze the data to see, first of all, if there was any appreciable difference in the rate of success with objects of these two radically different shapes, though no prediction was made regarding which type, the dice or the marbles, would yield the higher placement scores. In addition to this difference between the two groups, the experimenter was also interested in the total results as a further general measure of PK in the test. Also, in a part of the experiment a test was made of the effect of having the subject deliberately concentrate upon one type of object while ignoring the other. This was a type of dual-aspect task which the experimenter used in an earlier placement PK test and which gave a significant difference in scores, the consciously-accepted (primary) target yielding a positive deviation and the neglected (secondary) target, a negative deviation (1).

### Apparatus

The apparatus provided a throwing surface, 16″ x 24″, covered with a thin sheet of sponge rubber and surrounded by a 6″ wall. This area was divided lengthwise into equal "left" and "right" sides by a fine, taut wire upon the surface. The objects were released through a chute at the top of an inclined plane which delivered them onto the throwing surface directly in line with the wire. The chute and incline were both lined with corrugated rubber. The objects entered the top chute from a higher, V-shaped release-trough. Release was effected by raising the closed back end of the V-trough by means of an indirect pull-cord. The machine was mounted on adjustable legs and sat upon a sturdy table on a concrete floor, with one side of the table secured against a brick wall. In such tests as had to be conducted away from the "laboratory," it rested on the floor. The machine closely resembles that used by Cormack, as illustrated by Pratt (**4**).

### Procedure

Ordinary marbles and dice were used throughout three series, A, B, and C, a mixture of 24 of each type of object for each release. On each trial, the subject willed that the dice and marbles fall on one side of the dividing wire designated as the target for that trial. The experimenter then recorded the number of each class of objects on each side. Counting all objects on both sides of the wire assured against loss of any dice or marbles and against errors in counting. Each session co isted of 16 releases by the same subject (one record page of 32 runs).

Before each session a spirit level was used to see that the apparatus was level. To eliminate any error arising from any remaining bias, the target and non-target areas were alternated for each release. When any die landed upon the center wire, the direction of its tilt specified the area in which it was counted.

The experiment consisted of three separate series: Series A, in which the subject attempted to influence the dice and marbles equally; and Series B and C, in which one type of object was designated as the group to be influenced by PK (primary objective) on each trial, while the other type (the secondary objective) was ignored as far as possible. There was a brief pause halfway through each session.

## Results

The three series were all of the same length (448 runs each). The general trend of the results throughout the three series is interesting as showing the consistency of the scoring, but none of the series was independently significant.

### *Series A*

This series was begun in January, 1952, and it involved six subjects. The experimenter was his own subject in nine sessions, and each of the other five subjects took part in only one session. The results, given in Table 1, were recorded separately for dice and marbles, but the experimenter attempted to have all 48 objects fall upon the target side without any conscious effort to distinguish between them. Table 1, therefore, has no "primary" or "secondary" tabulation for this series as it does in Series B and C. The series yielded a negative deviation for the dice (−7) and a positive one for the marbles (+74), a trend that persisted through all three series.

### *Series B*

Series B followed in March, 1952. Nine subjects participated, and as in Series A the subject threw the 48 objects simultaneously on each trial; but this time the subject was instructed to concern himself only with the dice during the first half (8 releases) of each page and only with the marbles during the second half of the page. Accordingly, the results of Series B are shown in Table 1 in terms of the difference between dice and marbles as well as of the difference between primary and secondary objectives. Five of the fourteen pages were with the experimenter as subject. In this series the total results for dice show a deviation of −81; and those for marbles, +11.

### *Series C*

This series was begun in June, 1952, at which time 352 runs were completed. In December, 1954, an additional 96 runs were made to bring the series to the length originally planned. There was a total of 14 sessions, or pages, by 12 subjects. The only procedural change from that of Series B was that the subjects were permitted to select their own primary object for each half-session (exactly 50 percent were for dice); but their freedom of choice did

not appreciably affect the scoring. The two *types* of objects continued to show the same trend as that obtained in the first two series, with the dice being negative (−74) and the marbles positive (+17).

*Total Results*

Series B and C were undertaken primarily to test whether the difference between dice and marbles obtained in Series A would be confirmed. It is therefore proper, statistically, to combine the results of the second and third series on this point. The dice gave a deviation of −155 in 448 runs, and the marbles a deviation of +28. The CR of the difference is 2.50 (SD = 73.3; P = .012). Thus the suggested relationship of a difference in the direction of deviation between the two kinds of objects was confirmed in the last two series, where the statistical significance was due chiefly to the negative scoring on the dice.

Table 1

TOTAL RESULTS OF ALL DATA FOR PK PLACEMENT SERIES A, B, AND C

| Series | Data Divided According to Type of Object Used | | | | | | Data Divided According to Primary and Secondary Objective | | | | | | Total | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Dice | | | Marbles | | | Primary | | | Secondary | | | | | |
| | Runs | Dev. | CR | Runs | Dev. | CR | Runs | Dev. | CR | Runs | Dev. | CR | Runs | Dev. | CR |
| A | 224 | − 7 | 0.19 | 224 | + 74 | 2.03 | ..... | ..... | ..... | ..... | ..... | ..... | 448 | +67 | 1.10 |
| B | 224 | − 81 | 2.21 | 224 | + 11 | 0.30 | 224 | −49 | 1.33 | 224 | −21 | 0.57 | 448 | −70 | 1.35 |
| C | 224 | − 74 | 2.02 | 224 | + 17 | 0.46 | 224 | +20 | 0.55 | 224 | −77 | 2.10 | 448 | −57 | 1.10 |
| Total | 672 | −162 | 2.55 | 672 | +102 | 1.61 | 448 | −29 | 0.56 | 448 | −98 | 1.89 | 1344 | −60 | 0.82 |
| | $CR_d$ = 2.93 (P = .003) | | | | | | $CR_d$ = 1.21 (P = .23) | | | | | | CR = .67 (P = .5) | | |

Table 1 shows, of course, that the dice-marbles difference for the three series combined is significant ($CR_d = 2.93$; P = .003); and there is ample justification for attaching importance to this result, as the purpose in using two types of objects from the outset was to compare their results. The probability may be corrected for the two other evaluations applied (primary and secondary target difference and total score, neither of which gave extrachance results), but even when multiplied by three it is still significant (corrected P = .010).

## Discussion

The negative trend of the results with dice in all three series, and the accompanying tendency of the marble results to go positive, appear to indicate peculiarities in motivation. In other PK research, no appreciable differences have been found in tests with dice of radically different sizes or weights (3). Nor were any significant differences found by Forwald (2) in his placement tests using five types of cubes ranging from paper to steel. From the significant difference between the results for dice and marbles here reported, the shape of the objects seems to have a distinct bearing on the results, but not in a physical sense. The fact that the size of the deviation was approximately the same for both dice and marbles (though opposite in sign) suggests that it was no harder physically to influence the one than the other. The significant difference, therefore, would have to be interpreted as a *psychological* effect.

Any interpretation must be only tentative at this time, but the difference suggests that the experimental procedure produced a scoring effect that has recently been observed in other psi investigations. This is the tendency for the psi function to produce both positive and negative scoring in the same test when the task presents two distinct aspects. The contrasting negative and positive deviations may indicate that psi more easily finds overt expression when it can appear in this simple "disguise" of results which superficially appear to cancel each other out than when it must produce a straight deviation. In discussing the problem of psi-missing, Rhine (5) suggested that the use of a dual task may serve to channel the psi-missing tendency into the trials with one aspect. The subject would not be motivated equally toward each aspect, and it may be that only a slight difference in attitude is all that would be required to produce such a channeling of positive and of negative scores. The results of the present experiment appear to add to the evidence that a double-aspect test may produce psi-missing on one feature and psi-hitting on the other.

It is interesting to observe that a significant difference was obtained between primary and secondary targets in an earlier experiment, when these were the only two aspects involved in the test situation (1). But in the recent experiment, which presented a "double dual aspect" situation, only the difference between dice and

marbles was significant, while the primary and secondary objectives produced only a chance difference.

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# EDITORIAL

## THE RESEARCH IS THE THING!

Although it may appear a little ironic to be ruminating here from the editorial armchair about the relative need for more *doing of research* in proportion to related but secondary activity (including rumination), the topic identifies a real and important problem. Its choice is occasioned at this time partly by a noticeable spread of philosophical discussion of the results of the psi researches. Currently, for example, a symposium on psychical research is scheduled by the Eastern Division of the American Philosophical Association for its December meeting in Baltimore. Added emphasis has likewise been laid on the interpretative side of parapsychology by the *Scientific Monthly* in the publication and prominence recently given to Dr. Roland Walker's article "Parapsychology and Dualism." The last few years have witnessed international conferences of scholars active in, or ready to discuss and interpret, the findings of parapsychology. Recently published books, some written by philosophers and some by others, have more than ever before discussed broad questions of interpretation of the psi results.

All of these developments should be welcomed, of course, not only for the contribution each of them makes to the clarification and better understanding of the research results, but also because in the role of indicators, they represent a stage of development, a point of greater maturity, of the field of investigation. In any branch of science it is only after the smoke of battle over the main facts themselves has cleared that philosophical interpreters come out onto the field to see what new surveys are in order and what new alignments can be drawn.

This growing interpretative discussion is not, however, the only, and not even the greater, reason for the selection of the present topic. The investigation of psi has, normally enough, been accompanied by a load of paper work and incidental projects. Among the many aspects of a full-fledged field of research are the unavoidable and necessarily important ones associated with establishing and maintaining research centers, with arranging for funds, per-

sonnel, research facilities, and publication. Likewise, there are the various types of cooperation among co-workers to be established and maintained, differences to be dissolved and exchanges to be effected.

All such activities, like the philosophical discussions mentioned above, naturally owe their very existence to the basic researches; yet just as naturally, they all to a certain extent compete with these same essential research endeavors for time and energy and personnel. And that competition is not on even terms. These secondary activities have a way of becoming routinized and scheduled and thereby gaining certain priorities, since research, if it is to remain effective, can never be made routine. In addition, true pioneering research in the very nature of the case is comparatively difficult; the easy discoveries have all been made. Naturally, too, the results of basic inquiries are uncertain; no one knows at the start of a research whether or not it will be successful. Besides that, original investigations call for a high order of intellectual initiative. Each worker has to identify himself with a project and make it more or less his own creation if he is to generate the conative force required to carry it through to completion. All of these factors are part of the very nature of research.

The secondary activities, on the other hand (and why not consider editing as an example?) at least do not involve the kind of gamble the researches do. For these activities in general, the trail has already been blazed, the method and objective clarified, the going relatively easy, the result certain.

It is no wonder, then, that there should be a certain gravitation toward the armchair or the desk or the conference table. Doubtless, the same directional pull is to be found in any field of research. It is likely to be greatest, however, in a branch of inquiry in which the demands and uncertainties and difficulties are exceptional, as they are in parapsychology.

We believe we are justified, then, in sounding a note of apprehension for fear productive research activities will be proportionately crowded out as the other operations tend to become more inflated. It is not, however, that these other activities are in themselves in any way to be deplored; the question is entirely one of proportion, of the comparative share of the total energy expended over the

entire field that shall go to the continued production of new data. In any case, it is a question that should be considered and discussed, even at the obvious risk of momentarily adding further to the imbalance. If the problem is a real and urgent one, it is time to consider how the active fact-finding labor can be stimulated and encouraged so that a healthy ratio of it to the various other activities can be maintained.

* * *

If we proceed, then, to take this need for greater emphasis on research as an actual, important, and timely problem, the question follows: What can be done about it? Only very general suggestions can be made here, of course, for special circumstances surround each individual case. The few suggestions that will be offered may well render their best service in eliciting better counsel from others who may share the concern. Something may be accomplished, also, simply by raising the question; for then all who agree that the research is paramount, and who, in one capacity or another, take some responsibility for the guidance of trends and emphases in the field may revise priority ratings on the various activities. Such a fresh re-appraisal may in itself be effective.

Probably the most serious deterrent to the free range and expansion of active research in parapsychology is the prevalent misconception regarding standards and stages of investigation. We have already pointed out in an earlier *Journal* editorial[1] the serious handicap an explorer may labor under if he mistakenly fancies that he must do all his exploring weighted down with the full armor of precautions that legitimately belong only to the final stages of verification in a crucial research. While, of course, those same ironclad measures of safeguarding continue to be necessary for definitive tests, they are not only unnecessary but can even be downright handicaps when introduced prematurely in the first investigation of a new phenomenon.

The point suggested here is that if the merits of a more free and far-ranging kind of exploration were to be emphasized and the inquirer were given the necessary flexibility in selecting operating techniques, we could expect to have a great deal more of it going on than there is today. There can be little doubt that many

[1] "Some Considerations of Methods in Parapsychology," June, 1954.

would-be explorers have been discouraged from making a start by the formidable standards of experimental design, even though these may be appropriate and quite necessary later. Many more have suffered from the frustrations that were to be expected of an attempt to force those elaborate requirements upon the first tentative undertakings of an inquiry.

True enough, the science would not be greatly furthered by the addition of a whole raft of loosely conceived and hastily conducted exploratory ventures; but that is not the pertinent point here. Before there can be a harvest there must be a stage of quite useless seedlings. Without free exploratory beginnings we should expect a dearth of challenging projects for later development; projects that might ultimately under adequate conditions be carried through to final test. Not only that, but under such a handicap the research field must be expected to lose many of the inquiring minds it needs to man its widening frontiers. For the stimulation of more research activity, then, a better appreciation of the operative freedom required by the scientific explorer is needed and a greater relaxation of the defensive tensions that have understandably enough developed among psi researchers.

* * *

Still another idea may be suggested to help bring the productive research worker to the front of the stage. It might be a healthy encouragement to the exploratory worker to make more provision for the circulation and perhaps the publication of reports of provocatively interesting studies that do not claim to be definitive. We are not suggesting that the standards, let us say, of this particular journal be altered (although it is by no means inconceivable that a section of the *Journal* could profitably be devoted to what might be expressly *designated* as exploratory attempts). Rather, a mimeographed periodical might be developed for distribution to active workers and students of the field. Perhaps it would be justifiable to expand and adapt the *Parapsychology Bulletin* further toward this end. Speculating further, one could even imagine a special periodical developing primarily for the purpose of keeping fellow-explorers in closer touch with each other. The need of more mutual contact is an old and widely echoed refrain.

But no matter what develops, interest and enthusiasm thrive on

the sharing and exchanging of ideas, on the collaborations by which half-developed discoveries may be followed up to fully clinched conclusions. The facilitation of such exchange might provide the necessary stimulus to give desired priority to active research.

The aid we are considering need not, of course, be confined to the use of the *printed* word. Within limits there is an efficient role for the well-pointed-up conference between active workers over specific research problems on which effort is currently being expended. Visits, exchanges, internships, and such have their place when the research is already under way and there is a fair prospect of a fruitful return. Since there is no professional training school for the would-be parapsychologist, provision for such informal exchanges will have to be made as well as may be. In all such assistance to the potential contributor, however, the need is for orientation to be primarily on the research. A higher valuation than ever must be placed on the finding of new facts, new methods, and every new hypothesis that can be subjected to test.

* * *

One more suggestion: parapsychology needs to keep and appreciate its large amateur corps of workers. This branch of study has advanced so largely on the basis of amateur contributions that within our field the term makes very little distinction. It would be purely arbitrary to say when, if ever, a psi explorer became a professional. In any case, the work of the amateur still means a great deal in parapsychology. If a large group of free-scouting, nonprofessional explorers could do as much on the great project of the future, the winning of some measure of control over psi, as such workers did in the establishment of the case for psi, the prospect of victory over the problem would be enormously improved.

This call for greater emphasis on research, for a more favored priority rating for active inquiry, and for a freer, bolder, and broader range of exploration within and around the borders of our field may not make as urgent an appeal to all as it does to us who watch production with an editorial eye. But if our apprehensions are well-founded, the note we are sounding will be heard again and even again, and may well keynote the period of our science. The research *is* the thing.

J. B. R.

# BOOK REVIEWS

MODERN EXPERIMENTS IN TELEPATHY. By S. G. Soal and F. Bateman. New Haven: Yale University Press, 1954. xv + 425 pp. $5.00. London: Faber and Faber, 1954.

As a report to scientists this is the most important book on parapsychology since the 1940 publication of *Extrasensory Perception After Sixty Years.* If scientists will read it carefully, the "ESP controversy" will be ended.

The book divides roughly into three parts: a historical section, and two sections devoted to the ESP experiments with two successful subjects, Basil Shackleton and Gloria Stewart.

In the early chapters the authors present brief accounts and criticisms of most of the important experiments from the literature of extrasensory perception. The English viewpoint makes this section worthwhile reading even for specialists in the field; but the nonspecialist needs to be warned that there are errors and elisions, some of which will be mentioned below.

Most of the results with Shackleton and Mrs. Stewart have been previously reported in papers appearing in the U. S. and English journals, but this book provides the first convenient access to the work as a whole. The final chapter, "Science and ESP Research," is familiar to the reader, since it was given advance publication in this journal in December, 1953.

The work with Shackleton and Mrs. Stewart can be most easily grasped as a unified whole in terms of the basic card-guessing procedure that was used throughout, bearing in mind that many modifications were employed in the investigation of specific experimental factors.

Shackleton or Mrs. Stewart, acting as the percipient (P), was in one room, attended by an experimenter (EP), whose main function was to make sure that (P) synchronized his written guesses with the voice signals from the adjacent room. In that next room was the agent (A), who sat behind a screen and "sent the telepathic images" to the percipient. The agent did this by looking at one of five cards concealed beneath a box. Each card was printed with one of five letters of the alphabet, corresponding mnemonically to the initial

letters of the names of five animals. (The percipient made his response by writing down the letter of the card at which he supposed the agent to be looking.)

The agent was attended by a second experimenter (EA), who sat on the side of the screen opposite the agent but nearest the percipient's room. The function of the agent's experimenter was twofold: to signal the percipient when to make his guess (done by calling out the serial number of the guess), and to signal the agent what animal card to look at (done in an unknown code by holding a numbered card up to a hole in the agent's screen).

The details of the number code will be explained immediately, but the important things to fix in mind are that the agent's experimenter did not know which animal card the agent was looking at and that the agent never spoke during the test procedure. Therefore, the percipient could not have received verbal cues, unconsciously given.

The number code worked as follows. After the agent's experimenter had seated himself, the agent silently shuffled his five animal cards and laid them down in a new order. For his part, the agent's experimenter, before each call was made, obtained a number from one to five from a previously prepared random number list and held up to the hole in the screen a correspondingly numbered card. The agent, seeing, for example, the number two, would look at the second animal card counting from his left. In this way a prepared list of random numbers was converted, call by call, into a random sequence of five letters, by a code known only to the agent.

After fifty cards had been called, the procedure was interrupted, the card code recorded by (EA), and a new code adopted. At the end of the session the percipient's sheets of guesses were brought into the agent's room and, in the presence of witnesses, the percipient's guesses were decoded and duplicate records prepared. It is to be noted that the original written records of agent and percipient could be (and were) rechecked at later times.

As a rule, not more than four hundred guesses were made in an evening, and test sessions were held perhaps once a week. At the usual speed of calling, a card was presented every two to three seconds. Both percipients responded successfully at an average rate of better than 25 percent, where 20 percent would be expected by chance.

A comparative summary may serve to highlight the main features of the work. Basil Shackleton (B.S.) and Gloria Stewart (G.S.) were first tested by Soal in 1936, but owing to the unexpected and concealed way in which their ESP manifested itself, its occurrence was not suspected until November, 1939. Further tests with Shackleton were begun in 1941. Experimentation with Mrs. Stewart could not be resumed until 1945.

It may be inferred that the Shackleton tests were done to prove that ESP of the card-guessing variety could occur in England as well as in the U.S.A. Mrs. Stewart's work came later, and was carried out explicitly for the purpose of exploring further into the nature of the psi process.

The testing of B.S. extended from January, 1941, to April, 1943, and included forty sessions, in which approximately 12,000 card guesses were made. The last session showed highly significant results. It is mentioned that B.S.'s powers began to decline some months after the conclusion of these experiments.

The testing of G.S. began in August, 1945. There were 130 sittings, with more than 50,000 card trials. Mrs. Stewart's ESP ability had disappeared by the end of the summer of 1949, and tests with her were discontinued in January, 1950.

The personnel associated with Dr. Soal in the Shackleton work were for the most part fellow members of the (London) Society for Psychical Research, since these could give him the critical yet sympathetic support that he needed. By the end of the war Soal had already achieved considerable recognition from outside the field, and in the work with Gloria Stewart he believed it timely to begin collaboration with academic people generally, and especially with his colleagues from Queen Mary College of the University of London.

Mrs. K. M. Goldney was Dr. Soal's co-experimenter in the Shackleton work. Mr. Bateman collaborated in the Stewart experiments and in writing the book. There were numerous witnessing observers and more than a few different (ESP transmitting) agents. Shackleton was successful with three out of eleven agents; Mrs. Stewart with fifteen out of thirty. Bateman was a highly successful agent with Mrs. Stewart, but Soal was successful with neither Shackleton nor Mrs. Stewart.

In terms of the data, the most surprising feature of this research was the phenomenon of scoring displacement, the tendency for the

percipient to guess the card one ahead (+1) or one behind (−1) the card which was being offered by the agent. It was this effect in the 1936 exploratory data of both B.S. and G.S. that lay concealed for three years.

The progressive development of the displacement effect was different for the two percipients. In the preliminary work B.S. had shown a highly significant rate of success on both the (−1) and (+1) cards, and a negligible deviation from chance on the intended (0) card. When he began work in 1941 with a new agent, he showed only the (+1) effect. A year later when he resumed with one of his 1936 agents, he showed both (−1) and (+1) deviations and also a slight but highly significant deficiency of successes on the (0) card.

In the exploratory work by which she was discovered, Mrs. Stewart had scored positively on (−1), (0), and (+1) card positions, but significantly on only (−1) and (+1). Throughout her retesting, which began in 1945, she scored successfully on the (0) card only, regardless of agent, although for her data as a whole there were slight but significant negative deviations on both (−1) and (+1) cards. It is interesting to note that with one particular agent (Miss Elliott) B.S. scored only on (+1) cards, but with the same agent G.S. scored only on (0) cards.

It was discovered that when the normal rate of presenting cards (about one in 2.6 seconds) was doubled (to about 1.4 seconds per call), B.S. shifted his scoring from the (+1) to the (+2) card; or, when working with the agent with whom he normally scored on both (−1) and (+1), he would shift to (−2) and (+2).

When this rapid presentation was tried with Mrs. Stewart, she shifted from (0) scoring to (−1), an effect which is not of great interest in that it is most easily explained as a simple lag in response. In this connection, several experiments were tried in which Mrs. Stewart made no attempt to keep in step with the presentation of the card by the agent. Under these circumstances her scores fell to the chance level.

Several curious secondary effects (of the class known as "position effects") were observed when the data were later examined in detail. In the tests in which B.S. was scoring on both (−1) and (+1) cards, the rate of success rose sharply for those guesses in which the card sequence was such that he could be simultaneously

successful on both (−1) and (+1) scoring. For example, in the rapid rate experiments, guesses which were thus "multiply-determined" for (−2) and (+2) scoring were successful 58 percent of the time; while non-multiply-determined guesses at the rapid rate were 29 percent successful (expectation being, of course, 20 percent). This reinforcement effect was found in the 1936 data of B.S. as well as in his later work. It was not found in the exploratory work with G.S. (the only time she showed displacement success).

Mrs. Stewart did display very pronounced position effects of her own kind involving guesses where the target card happened to repeat itself. Chapter XIX is devoted to the description of these anomalous patterns of success.

Still another effect found in the data of G.S., but not at all in the data of B.S., was the tendency to have "streaks of luck" (known mathematically as "runs"). In Appendix D it is shown that the observed run distribution in the data of G.S. has a chance probability of $10^{-14}$.

Another secondary effect known as "salience," in which the scoring rate varies within the structural organization of the experiment, is by now a well established feature of psi. In the Shackleton records there was no salience save for a slight (but significant) decline from the first to the last page of data within the test session. In the Stewart data, on the other hand, salience effects are marked, and extremely so for segments of the columns of the data page.

These differences in response patterns must be taken to indicate subtle differences in the personality structure of the percipients. It could be that they are in the nature of finger prints, identifying, but essentially meaningless. It is more likely, however, that eventually we shall understand why some percipients respond one way, and some another. Indeed, in many instances it is possible now to see either a motivational or a Gestalt rationale in the observed patterns.

Among the problems briefly investigated were: (1) the influence of metallic lead screening (no influence); (2) percipient's awareness of ESP success (unaware); (3) response to repetitive instead of random card arrangements (percipient successful); (4) the use of cards where the odds varied from 1 in 2 to 1 in 52 (success achieved at both extremes, but 1 in 5 is probably an optimum).

Major experimental emphasis was given to several problems, including the effect of a long distance between percipient and agent.

The London-Antwerp tests with G.S. will interest the ESP skeptic for their evidential value. But they will especially interest the parapsychologist in that they were highly successful even in instances where the agent was not at the promised street address.

In certain other tests, with B.S., the agent's experimenter used colored counters in place of random numbers. The counters were drawn by hand from a mixed supply of five colors. The fact that B.S. was responding (especially in the double-speed tests) to a card still to be determined by a counter which the (EA) had not yet touched raises in new experimental form the question of precognition—an old question, to be sure, in the literature of parapsychology.

Another important modification of experimental procedure occurred when it was found that it was not necessary for the agent to look at the cards, but merely to touch their backs, providing the agent had previously known the order in which they were laid down. This modified procedure was successful in cases where the agent, after shuffling the cards, had looked at them for as little as five seconds and subsequently had no conscious memory of their order.

It should be noted that all attempts at clairvoyance experiments (in which no one knew the card order) invariably failed with both percipients. This, of course, is in marked contrast to the experience of many other experimenters. On the other hand, experiments in "pure telepathy" were successful. (In these telepathy experiments a secondary code was used but never objectified.)

Among the more important findings of this book are those connected with the simultaneous use of two agents. In some cases the agents were "in opposition"; that is, they worked with separate codes, so arranged that a successful guess with one agent was necessarily a failure with the other. Again and again, under a wide variety of conditions, it was found that the percipient fastened his or her responses onto the cards of one agent to the exclusion of the other, and that the favored agent was the one to whom the percipient's attention had been directed in cases where direction was attempted.

Still more interesting was the use of "split agents," in which one agent shuffled the cards, looked at their order for thirty seconds, turned them over, and sat with folded hands, while behind another screen at another table, a second agent watched the random numbers and pointed to blank cards lying in front of her. Using this scheme Mrs. Stewart responded successfully with four out of five agent-pair

combinations. Ostensibly, she was able to combine information from two minds in making her guesses.

A book that reports more than a decade of statistical experimental work in a science that is still without theory is perhaps the hardest kind of book to write. No work of this scope can be expected to be without weakness, whether real or imagined by the reviewer. But those weaknesses are remarkably few in the case of *Modern Experiments in Telepathy.*

It is perhaps unfortunate that the authors dignify the concept of "antecedent improbability" as applied to a new phenomenon (page 11). The idea that generalized experience should be weighed *in the same scale* as directed experimental evidence has been used by less imaginative men to deny the validity of almost every important discovery in science.

A related point concerns an experimenter's choice of level of statistical significance. On page 42 the authors state: "We should, however, employ such a low grade of significance (odds of 200 to 1) only when making comparisons involving small blocks of data taken from a much larger whole whose consistent significance is beyond all dispute."

This statement in context is obscure. The paragraph preceding it might suggest that the authors were referring to situations where the experimenter kept a running count of success and determined his stopping point thereby. The paragraph which follows makes clear that the authors are not concerned merely with optional stopping, but are making a much broader recommendation. But if so, a new obscurity arises. The authors fail to say why odds greater than 200 to 1 are *needed* in parapsychological experiments. The question is a methodological one that appears again and again in this book. Now it seems to be answered one way and now another, but it is never adequately resolved. Obviously, if one hopes to convince a skeptic by means of a single experiment, high odds may be more effective than low. But if one's objective is to get on with the business of science, an experiment yielding odds of 100 to 1 or even 20 to 1 may in many cases be regarded as successful.

The authors might have shown more circumspection in finding fault with the research of others. Their own discoveries shine brightly enough without need to create the impression that nearly all previous experiments in this field were somehow not quite adequate.

A case in point is the 1920 work of Heymans, Brugmans, and Weinberg. Although the expected rate of success was 2 percent, the achieved rate (e.g., in the series where the guesser took alcohol) was as high as 75 percent. The accounts in English of this research agree that a hole was cut in the floor (between the upper room with the experimenters and the lower room with the subject) and that two sheets of glass were inserted with an air space between, so that a shout in one room could scarcely be heard in the other. Soal and Bateman say (on page 16): "It is only fair . . . to point out that the experimenters were highly competent psychologists, and would naturally be on their guard against the possibility of auditory cues." Yet on the preceding page the same authors say: "It was altogether unfortunate that the experimenters who knew the chosen square, watched [the subject] while he was groping over the board [in the room below]. Quite possibly they became excited when his hand was in the region of the right square, and they may have made some slight movement which afforded an auditory cue. Whately Carington did not think this possible, but we cannot share his optimism, even though he had visited the scene while we have not."

Admittedly, it is difficult to feel confidence in an ESP experiment done a generation ago in a foreign land. But if there is a weakness in this Dutch experiment, Soal and Bateman have not put their finger on it.

This reviewer was surprised to find, on page 47, the Pratt and Woodruff experiment mentioned only as a "fairly good series." Workers in the U.S.A. generally regard it as one of the most carefully performed experiments in the literature of parapsychology. This experiment is described in detail on pages 56-57, and there the mystery is clarified. Soal and Bateman say: ". . . it might have been possible for the guesser to get a glimpse of the card as it was being placed by peering through the slit" at the bottom of the screen. Soal and Bateman describe the screen, but fail to say that the slit at the bottom was protected by a special baffle. On page 124 of the original paper, Pratt and Woodruff state: "On the experimenter's side of the screen, 3½ inches back of the aperture was a low vertical screen three inches high and twenty-three inches long. Its position in relation to the aperture was such that the subject could not possibly see the cards held by the experimenter." Pratt and Woodruff's claim is amply supported by the photograph of their screen, appearing

as Fig. 1 of the original paper. Perhaps Soal and Bateman were confused by some of Soal's own experiments (page 112) in which a slotted screen was used without a baffle.

In disparaging the scientific work of others, one should be as explicit, fair, and cautious as possible. On page 348 the authors refer to "the questionable experiments" of Riess and of Carpenter and Phalen, "which in the present book we have scarcely considered to be worthy of mention. . . ." Nothing is said there as to why these experiments are "questionable" and the index provides no other references. However, this reviewer did find a specific criticism of the Riess experiment on pages 47-48. Soal and Bateman point out (1) that the unwitnessed results are dependent upon Riess's good faith, and (2) "that the guesser might have gained access to the records (left lying in a drawer) before they were checked by the investigator." Now as to the first criticism, there is nothing in the tradition of Western science that requires that an experimenter have witnesses to vouch for his honesty. At most, Dr. Riess might be accused of being unrealistic if he expected other scientists to accept his results. As to the second criticism, Soal and Bateman should have gone on to say that the drawer in which the records were left lying was in the experimenter's own home, and that the guesser was a stranger who did the guessing in her home. The authors should also have pointed out that it would have been necessary for the guesser to have gained access to Dr. Riess's home repeatedly over a period of months. One suspects that the real objection which these authors (and many skeptics) have to Riess's work is the fact that his subject showed the highest sustained scoring rate in the history of ESP. This reviewer, for his part, wonders whether this startling and persistent demonstration of ESP was not in some obscure psychological way, made possible by that casual attitude which the experimenter felt toward the results as they continued to accrue.

It is evident that the authors have a strong adverse feeling about group experiments (by which they mean experiments with unselected subjects in which the results are pooled). They make two specific charges against this kind of experiment, as well as a number of vague criticisms. The first charge appears on page 60 where the authors say: "We are quite convinced that a great many group demonstrations of ESP are spurious, being due to optional stopping."

No examples are cited, and indeed it would be difficult to find examples in the scientific journals.

The second charge (page 62) is that group experimenters engage in *ad hoc* analysis in order to demonstrate ESP. Again, no examples are cited. Since the authors specifically exempt the work of Humphrey and of Schmeidler, and by implication the group work of others which they discuss favorably elsewhere, it is clear that their criticisms are not aimed at the vast majority of the group experiments published in the scientific journals. The sarcasm of this criticism (page 62) is all the more surprising since it comes from men whose own work has been subjected to similar generalized and unsupported attacks.

The authors do single out two group experiments for criticism. In the first they quote the conclusion from a paper by A. A. Foster as follows: "Finally it may be said that at least one group of American Indian children have given scores in ESP card tests that are ascribable only to the ability known as extrasensory perception." Subject to the implied limitation that the results may have been due to chance, this conclusion seems to this reviewer to be a reasonable one. The experimenter has cautiously avoided a generalization about the population of American Indian children. Nevertheless, Soal and Bateman say "Surely this conclusion is unjustified." Then, without specifically supporting their opinion, they go on to complain (1) that the individual scores were not discussed, (2) that more experiments should have been conducted, and (3) that there is no certainty that the experimenter was competent.

The authors appear to assume in this section that if ESP can be detected in the combined result of short tests taken by a group, it should be detectable in longer tests given to individuals of that group. It should be obvious that for many subjects the boredom of long tests could prevent the continued emergence of the phenomenon. But the real sin of the authors is to take *any* unjustified assumption about the nature of ESP as a basis for impugning the work of other scientists.

In a similar vein, the authors complain, about a paper by Shulman, that "not one" of the individual scores among a group of institutionalized psychotic subjects showed evidence of ESP, although the group taken as a whole reached the .01 probability level of significance after correction for selection. The authors go on to say:

"Moreover, there is no attempt whatever to continue experiments with individual members. . . ."

If an experimenter has the good fortune to encounter a Shackleton or a Stewart who is available, willing, and able, he will, if his other obligations allow, continue to perform experiments for several years as Soal and Bateman have done. It is hard to see the point of the criticisms offered in connection with the papers by Foster and Shulman. Their experiments were done as well as possible under the circumstances. They are minor pieces of research, reasonably reported and evidently worthy of publication. It is not suggested that they involve either optional stopping or *ad hoc* analysis. Certainly, these papers do not make a case for the wholesale criticism of group experiments. Soal and Bateman do not always seem to appreciate in dealing with the scientific work of others that a paper is to be judged by its pretensions.

Two ideas occur as leitmotifs throughout the book. The one is "unconscious whispering"; the other is "statistical artifact." To attempt to explain the data of Shackleton and Stewart as the result of unconscious whispering is ridiculous if one considers how the experiments were conducted. The repetition of this theme becomes tiresome, and it will not surprise this reviewer if some skeptics accuse the authors of knocking down a straw man the better to distract the reader from other defects of the work (whatever they may be).

The supposition that the significance of these data can be explained as a statistical artifact arising from fundamental defects in probability theory is equally ridiculous, but the repeated illumination of this question is not without point. Many academic scientists have little understanding of the fundamentals of statistical method and are easily misled by irrelevant speculations such as those of G. S. Brown.

If one leaves aside the reporting of the work of others, there is little to be criticized in this book. Several interesting methodological points might be raised. The authors are unwilling to reject the possibility that ESP might be explained by a "physical radiation theory" (page 304), although they agree that none has been offered which even remotely meets the experimental requirements. This broadminded attitude is commendable, but one is entitled to ask what the authors intend by the words "physical radiation," if one excludes

not only all currently known forms of radiation but also the space and time relationships which characterize the known radiations. Whatever the ESP agency may turn out to be, it can be called a "physical radiation" if one chooses, since both words appear to be completely undefined. What is more, they are "good" words, unlikely to offend anyone in the atomic era; so by all means let us use them.

Some of the procedures of Chapter XV strike this reviewer as ludicrous, but he hastens to concede that this is a matter of taste in which, judging from work on both sides of the Atlantic, he is in the minority. In order to prove that "pure telepathy" (as defined) is possible, the authors must be careful throughout their lives never to speak the key words which give the code to the cards which were guessed in this part of the research. Otherwise, of course, precognitive clairvoyance, rather than telepathy, becomes a possible explanation for the results achieved.

It is worth while to consider the nature of the reasoning that led our authors to this precarious predicament. By definition and the use of the excluded middle, clairvoyance and telepathy are created as distinct categories. One may then proceed to determine whether one or both phenomena occur. In a try for extreme rigor, one is led to the kind of telepathy experiment here described. To pry from Nature her secrets by a little experimentation and much logic is in the Greek tradition, but a reversal of emphasis has proved more fruitful since the Renaissance.

In Appendix F. the authors report briefly some ESP group experiments supervised by G. W. Fisk in England. The authors point out that Fisk achieved striking forward and backward displacement scores and thus, like Whately Carington, can apparently evoke ESP among unselected subjects. On the other hand, Denys Parsons, Donald West, and S. G. Soal have had no success at mass experiments. Now the striking feature about the Fisk experiments is that there were actually many experimenters involved, and Fisk's only contact with the experiment was to organize it and process the data. Since all of these named English workers are friends of long standing, the authors accept these results, find them puzzling, and as a solution, offer what may be the most important speculation in their book.

They suggest that Mr. Fisk "as the person in supreme control of the assistant experimenters and the multitudinous details of the tests, . . . may not only have organized the experiments but in addition [may] have been a deciding factor in dictating unconsciously the general [displacement] trend of their outcome." The authors go on to suggest that it may "have been the subconscious of S.G.S. which determined that neither Basil Shackleton nor Mrs. Stewart should succeed in clairvoyance tests." This reviewer would like to add a speculation of his own. Is it possible that both the original appearance of displacement scoring in the Shackleton and Stewart experiments and the unprecedented and spectacular continuation of those experiments may have involved the psychic ability of S.G.S. as well as of his subjects? In Part III of the *Combined Index* of the Society for Psychical Research under the name "Soal" one finds listed, among others, the "Veley Script." A study of this script, written under the pseudonym "Mr. V." in Volume 38 (1929) of the *Proceedings,* will convince the reader that Soal has had some extraordinary psychic experiences of his own.

That the personality of the experimenter is important to the success of psi tests is not a new idea. The situation was nicely summarized in an editorial footnote in the *Journal of Parapsychology.*[1] But the possible pervasive influence of the experimenter as suggested by Fisk's results comes as a surprise.

The methodological significance of these speculations is greater than might at first be supposed. On page 51 ff. Soal and Bateman say: ". . . the Shackletons and the Stewarts are hard to discover. . . . One will certainly not find them by sitting in a psychology laboratory, twiddling one's thumbs and waiting for them to turn up. . . . The experimenter in telepathy must have drive and energy. . . ." This is all sound advice, but the authors go on to say: "We feel convinced . . . that almost anyone whose mind is not completely closed to the possibility of ESP, or who is not animated by a desire to disprove it, can, with sufficient patience and hard work, obtain satisfactory evidence of its occurrence, after perhaps some initial disappointment."

Fair words, and perhaps true! But no one who knows Denys Parsons (to pick an English example) will accuse him of lacking patience and industry. Perhaps the experimental problem is not as simple as the authors suggest—there is by now considerable evidence

[1] Volume 2 (1938), p. 236.

to suggest that it is not. If so, no useful purpose will be served by pretending otherwise.

In an organizational sense, this book suffers from the limitation that it is simultaneously intended to convert skeptical scientists, to interest intelligent laymen, and to inform parapsychologists about the more intricate details of the research. The reader who is already convinced that ESP is impossible will have little difficulty in losing himself in the wealth of detail which is provided. It might have helped if the authors in their preface had directed the skeptic to the more evidential parts of their book (e.g., to pages 120-149, 192-198, and 284-294).

Another point needing emphasis is that one should not assume that the new findings about ESP as reported in this book are generally true, but only that they are characteristic of the percipients tested. Most of the findings are not new, of course, but merely confirm the work of others. They are nevertheless extremely worth while in that some features of ESP hitherto considered as only highly probable can now be regarded as established.

The book which Soal and Bateman have written is readable, well indexed, and remarkably free of typographical errors. With only a little rewriting in its second edition, it will become a classic without peer in parapsychology.

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New Dimensions of Deep Analysis: A Study of Telepathy in Interpersonal Relationships. By Jan Ehrenwald, M.D. London: Allen and Unwin, Ltd., 1954. 316 pp.

The gathering momentum of psychoanalytic interest in paranormal communications between patient and therapist, or between patients and others in their interpersonal world, was evident last year at the First International Congress of Parapsychological Studies at Utrecht. While Freud had repeatedly raised the question of telepathic material in psychoanalysis, observations were scarce until recently.

Dr. Ehrenwald's first volume, *Telepathy and Medical Psychology,*

reporting his own observations and those of other therapists, appeared in England during the war years. It was followed by a series of papers from several analysts presented to analytic groups in New York. The number of such studies is now large enough to have warranted the editing of a volume devoted to this subject by George Devereux last year.

To mark out with a few strokes the character of Dr. Ehrenwald's volume, the first thing to be said is that the book is charmingly written with a highly felicitous use of vivid imagery and analogy. It is a very human and readable document. Secondly, we must regretfully note that the *amount* of telepathic material appearing in the work of a sensitive and busy analyst, who is constantly on the lookout for evidential material, is small. To be sure, a great deal which is possibly telepathic goes unobserved. But we have in this volume what appears to be the total yield of a period of about a dozen years, since the earlier volume, in a professional life which must include at least thirty or forty hours of analytic work per week. The number of dreams offered as paranormal is less than twenty. The question of the quality is an independent one to be taken up later. This quantitative issue is, however, of importance to us, partly because it relates to the question whether psychoanalysis is an effective way of gleaning paranormal material, and partly because it raises doubts whether a yield in this small quantity is likely to have a massive effect upon practitioners who are concerned primarily with what they themselves can observe, use, and integrate in their analytic thinking. A third point about the book is its excellent integration of case material and theoretical speculation, with an easy movement back and forth, keeping the reader in touch with the patients whose lives are described, and at the same time holding up before our eyes the almost dazzling conception of a new world which will come as deeper insights into human nature are gleaned.

## II

From the reviewer's point of view, the book must be evaluated both with respect to method, and with respect to interpretation of the data. In the matter of method, the author presents on page 35 the following comparison of three ways of achieving scientific knowledge:

Table 1

| Group | Data | Method | Criteria | Nature of Material |
|---|---|---|---|---|
| I | Quantifiable material, e.g. Rhine's card-calling tests | Mathematical assessment, probability calculation | Statistical significance | Quantifiable |
| II | Names, dates, numerals, combination of distinctive features | Phenomenological description, authentification of evidence | Uniqueness, "tracer effect" | Cognitive, ideational, unequivocal |
| III | Meaningful relationships, "intelligible constellations" (Kardiner) | Psychoanalytic; "comparative analysis" of agent and percipient | Psychological significance, consistency | Symbolic distorted, largely preconscious |

The Roman figures (horizontal rows) indicate three types of evidence or scientific data on which research into telepathy can be based. The vertical columns indicate the principal characteristics of the data.

This comparison is useful. However much the statistician may object to a conviction which arises from a congruent set of observations, none of which can be scrutinized in terms of theory of probability, this is the oldest method of science, and one which still is relied upon heavily in many of the established sciences. Take the question of the "origin of species" from a primordial living stuff, the theory of the transmutation rather than the independent creation of the various species. There is, in the nature of the case, only *one* evolutionary sequence on this earth; there are no P-values. One looks at all the evidence as Darwin did, and one says, putting it all together, "It makes me believe so-and-so." One looks at the Grand Canyon of Arizona and decides whether it is credible or not that the sea once raged where today fossil marks line the Bright Angel Trail. One cannot gather instances of Grand Canyons of Arizona and apply P-values; one takes all of the evidence as best one can and makes a judgment. This has been the method of choice in clinical medicine to this day, where the individual patient exists only once and can continue to exist only if a correct judgment from data is made.

When, however, Dr. Ehrenwald goes on to contrast the statistical method in this sharp fashion with the method of "meaningful relationships," he appears to be unwilling to recognize the commonsense nature of statistical operations. When there is a logical,

orderly, and acceptable reason for believing something and the P-value comes out .01, we say it is significant; we regard the hypothesis as tentatively established; and we move on to a new achievement. If, however, the result is in the *unexpected* direction, we re-examine the problem and set up a new method, revise our theory, gather more data under the same or better conditions, or do something else which will make us likely to achieve, on the second replication of the experiment, something which gives a P-value in harmony with our notion of the sense of the situation. Ultimately we expect statistical data to make sense; that is, we expect that behind the statistical façade there will always be intelligibility. The many objections to parapsychological data to the effect that they are simply unexpected or statistically improbable occurrences would, as a matter of fact, be fatal objections if there were no orderly, logical, and reasonable conception which parapsychology had to offer. Dr. Ehrenwald seems to have been misled here into a conception of a statistical method which is blinder even than the conception of piling up the facts and seeing where they lead. There has to be a theory and the theory has to be tested. Statistics is one of the ways of testing a theory. Congruent evidence is another way of testing a theory. Basically, there has to be a theoretical structure behind either one.

As a matter of fact, the two are usually supplementary rather than opposed. It is to be doubted whether this device by which the two methods are opposed actually achieves the purpose intended. Neither can it be claimed that the method of congruence is self-sufficient. Usually the congruence procedure is sound only if to some degree statistical conceptions can be brought in. If one goes back to the illustration drawn above regarding organic evolution, one will find that one buttresses the argument from congruence by reference to aspects of evolutionary change for which P-values *are* obtainable.

As to the second method which the author offers—the method of analysis of "tracer" effects through meaningful expression of a telepathic interchange—one must of course agree with his observation that the method is sound *only* if the method by direct comparison of manifest content with reality is carried through. Suppose, however, that a certain amount of manifest content is actually in harmony with known facts. How far does this permit the pursuit of symbolic or associated material? This would appear to depend entirely upon the degree of freedom which psychoanalytic method in general is

allowed to bestow on the individual investigator. In all psychoanalysis, from the beginning, it has been a question of the disciplined and the indoctrinated viewpoint of the therapist which has determined whether the associations given are indicative of one or of another reality in the patient's life. All associations are, of course, "valid" in their own way; but the therapist "stops" at one point, or indirectly encourages the patient to go further, and gives by countless hints, postures, vocal changes, or even silences, cues as to what the patient is to continue to think or not think, feel or not feel. This has always been one of the great problems in doing research on the psychoanalytic process, whether one is concerned with the problem of the paranormal or not. Not only are associations often supplied by the analyst; so too are the complex interpretative structures which are erected as scaffolding or even as structural steel for the final meaningful interpretation to be presented to the reader. It is, after all, nothing more than the experience, the attitude, the outlook—in short, the total personality of the analyst—which supplies the key; and if it be true that different analysts find in their observations confirmation or refutation not only of parapsychological theories but of other theories, can we say when a hypothesis has been adequately tested? It happens that the observer, in this instance, is highly intelligent, adroit, flexible, imaginative, and reasonable. Does this mean that the method upon which he concentrates can be duplicated by anyone else? So far as we can see, it does not.

The documentation through use of the best of the telepathic dreams is clear and extensive, the verbal material being supported by drawings and photographs, and when necessary, by independent corroborative statements by other persons in a position to know the facts. Space precludes the possibility of describing all of them. (Some of them would take several pages to recount.) But I will reproduce here from pages 76-80 material showing the rather extraordinary nature of a telepathic dream, one of a series appearing to display unconscious information about the psychiatrist's living quarters. The patient says:

"I was fixing wires or pieces of string attached to each other. I pulled the strings so that the light would work again. I had the impression that the room was changed or that something had to be remodelled in it. I had to crawl into the place, I don't know why. It had a floor sloping towards the centre. In the middle there was a

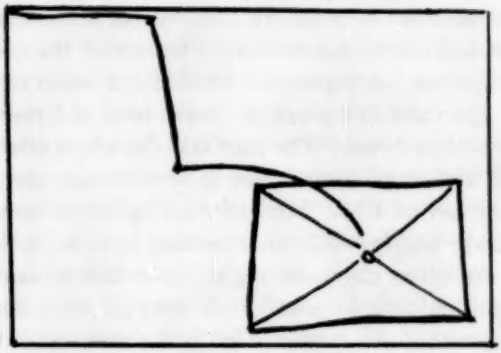

FIG. 4. Mother's room with drain-pipe. Tracing of patient's pencil sketch.

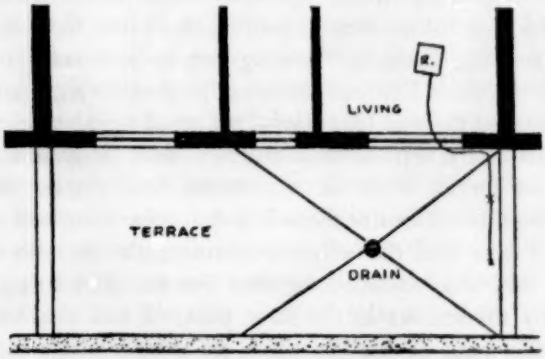


FIG. 5. Floor plan showing living room, radio with aerial cord, sloping terrace floor and drain-pipe.

drain-pipe, perhaps to collect rainwater. I found it strange to see a drain-pipe in the middle of a room."

Dr. Ehrenwald comments, reproducing the sketch: "The patient's sketch shows the rectangular vestibule with the drain-pipe in the centre and to the left the outlines of what he described as his mother's porch, with the staircase leading up to it. It will be noted that the chief emphasis of the drawing is placed on the rectangular box-shaped vestibule. The zig-zag line on the right is drawn with equal emphasis. It represents the electric wire or cord which the patient was supposed to fix for his mother. The drawing also indicates two points on which the cord is fixed to something—the patient could not tell to what. . . .

". . . his facetious reference to the possible telepathic implication of his dream was not mistaken. As it happened, the evening before the dream occurred, on September 14, 1950, a radio repairman had been in my apartment to repair a chairside model radio set which had recently broken down. The man said there was not much wrong with it. All that was needed was a new outdoor aerial so as to improve reception of FM. He had also tightened some screws to secure the power supply. All this I learned from my wife on coming home from the office the same night. She had already purchased the aerial cord and asked me to attach it to the set according to the repairman's advice. As seen in Fig. 5, the radio is situated in the living room which opens through a french door and two windows to the terrace belonging to our apartment. I led the wires through the open door on to the terrace, turning them in a right angle to the left and fastening them by drawing-pins to a wooden box in the corner. From there I turned the wires in another right angle to the left and fastened them at two places to a small wooden fence separating our part of the terrace from our next-door neighbour. . . .

". . . the terrace itself has a concrete floor sloping toward the middle. Slightly off centre there is a drainpipe to collect rainwater. I must add here that the wires constituting the aerial were not in one piece; they were fastened together like pieces of string at points indicated by the crosses in the floor plan. I had also to do much pulling of the wires during the whole procedure, partly because they were not gripped tight enough by the two antenna screws, partly because they had to be conducted outside the door under the door-mat. As can be readily seen from this description I am no expert in fixing broken-down radios. On the contrary, the job was done with considerable fumbling and fussing and was only persisted in because both the present writer and his family were most anxious to have the radio in working order so as to be able to enjoy a particular concert which was to be on the FM programme that evening. . . .

"This combination of seven or eight distinctive features in both dream and reality, in conjunction with the temporal coincidence, is striking enough to be suggestive of telepathy. Indeed, some of the details contained in the manifest content of the dream illustrate what I described as the telepathic tracer effect better than any of the incidents reviewed in earlier chapters. In short, the dream described

here seems to meet the criterion of specificity or uniqueness as well as can be expected from any spontaneous incident of this type."

A more complex case relates to the dream of a woman whose life was marred with suspiciousness and depression. She dreams she was living in a penthouse. She received a visit from two friends. The scene changes and she is in her cousin's house looking at a brass tray bearing a card on which something is written in Spanish. The scene changes again and relates to the husband's driving to the back of the house rather than the entrance. As a matter of fact, two friends, Louise and Caspar, have been the psychiatrist's guests on the night of the patient's dreams, the only time they had been in his house. They had been delayed by a taxi driver who, insisting that he knew the way, had arrived at the back side of the apartment house. One detail could not at the time be explained: the patient had no associations that threw light on the calling card with the Spanish words. A few weeks later the psychiatrist's daughter noticed a little match box in a copper tray which Caspar had left, on which was inscribed in Spanish the name of the maker. My condensation leaves out some confirmatory features; the illustration is given chiefly to make clear that the material appearing in the dream was not all in the possession of the psychiatrist, and to fortify the author's point: "in this case the telepathic dream was based on information derived from more than one telepathic agent." (pp. 205-206)

III

Now as to the major contribution to theory: Telepathy is conceived in evolutionary terms as a primordial process linking the individual with those significant to him and, in some instances, linking groups together in a dynamic whole. Such interpersonal contacts spring—in large part and perhaps exclusively—from the needs which can only be satisfied through such contact. The telepathic process is moreover regarded as closely related to normal sense perception and is frequently capable of combining with it in making life adjustments. This very fundamental point of the interdependence or perhaps even fusion of normal with paranormal processes has been almost invariably neglected by experimentalists, and indeed even by clinicians who have cast a glance at parapsychological research. This conception of fusion is, of course, related to a broad conception of

the unity of the organism which is repeatedly presented in different contexts throughout the book.

The author stresses that the emotional needs—conscious or unconscious—which the psychiatrist experiences with reference to the patient (counter-transference phenomena) are a large part of the problem—at least they supply a context for the patient's reaching toward the psychiatrist and in all probability contributing dynamically to the quality of the interchange. In Dr. Ehrenwald's terms, "Telepathy may occur on the exact point of intersection of two corresponding or converging emotional trends in patient and therapist." (p. 60)

This conception of the mingling of the two forms of communication is not fully worked out and it confuses the reviewer. Consider the following (page 49): "More often than not telepathy may be indistinguishable from the give and take of the familiar means of human communication. Words spoken and unspoken may assume a deeper meaning by subtle inflections or intonations of voice, by unconscious expressive movements—and their unconscious appreciation—without telepathy being involved in the process." Having pointed to this normal (not paranormal) process of communication, it is remarkable to find on later pages that Dr. Ehrenwald seems convinced that all communication not using verbal means must necessarily be telepathic. Thus when he thinks a patient's dream may have referred to the therapist's activities, he makes sketches which indicate a number of different scenes, one of which is relevant to the therapist's experience; then the patient, *in the psychiatrist's presence,* picks out the picture most meaningful to him. This method invites the operation of the normal type of communication through use of slight normal cues which Dr. Ehrenwald has just described and which, he rightly points out, need not involve telepathy.

This failure to use the principle of parsimony is disturbing to this reviewer. Thus for example unconscious dynamics relating to guilt are offered by the author as mediated in some degree by *paranormal* processes. He observes that even in the absence of direct evidence of castration threat, the child may nevertheless develop castration anxiety. Dr. Ehrenwald believes that feelings and ideals related to guilt may be telepathically conveyed. Again, the only question to raise is: What is the evidence? It seems to be scarce

indeed, and the interpretations of such phenomena through the normal unconscious signal systems already described by Dr. Ehrenwald seem more simple and satisfactory.

In discussing the way in which patients may produce material reflecting a special interest of the psychiatrist, Dr. Ehrenwald goes on to point out that telepathic leakage probably predetermines to some degree the dreamer's expectation as to what kind of dream he should dream for the benefit of his analyst. Then this sentence: "One could object at this point that all these discrepancies simply reflect changes within the general outlook of both patient and therapist, reinforced by their community of interest and intellectual preoccupations. This is a perfectly legitimate objection—but it only holds true for cases in which the transmission of specific material from the therapist to patient can be explained in terms of overt verbal communication." Again, why?

One might assume that from this starting point individual telepathic incidents which make plain the preoccupation of the patient with the physician's life activities might lead on into a systematic personality study of the gifted individual; that is, of him to whom telepathic phenomena are a constant or profound expression. While Dr. Ehrenwald's earlier book did give considerable attention to this problem of the specially gifted, the problem is not further considered here. It is, of course, clear that some of the morbid phenomena shown by some of the patients are related to their special needs and therefore to their specific telepathic performances. But it is not clear in what way the character type relates to the gift in general—whether, for example, a paranoid type is more predisposed than a compulsive type—and it is by no means clear that the needs of disturbed personalities predispose more towards telepathy than normal needs do, as in many spontaneous cases of telepathy. One sees here the imprint of Dr. Ehrenwald's own specialty rather than a view of telepathy geared to a broad conception of everyday living.

## IV

As a medical man, the psychiatrist is frequently reminded of analogies between psychological and bio-chemical processes which, offered as analogies, seem to be quite useful. Of special interest, for example, is the conception of anapsychic organization and catapsychic breakdown, comparable to anabolic and catabolic processes in chemis-

try. "Unless such foodstuffs as proteins, fats, or carbohydrates are broken down into smaller molecules before passing through the mucous membranes of the digestive tract, they may cause allergic or anaphylactic reactions. The same is true of the intrusion of heteropsychic material into the autopsychic sphere. It may either be broken down and assimilated by the ego, causing no reactions, or barely noticeable ones; or it may meet with resolute resistance and be altogether eliminated by the ego." (page 141) The material articulates with the complex psychopathological theory offered in Dr. Ehrenwald's earlier book relating to the elaborate devices by which the ego may preserve itself to the exclusion of the unconscious danger signals within the person himself, and ward off, at the same time, elements coming from other persons which might constitute a threat to his integrity.

The author emphasizes current studies of *empathy* and *enkinesis*, the former relating to the process of making oneself subjectively one with a person, sharing his subjective states, and the latter relating to the process of carrying out the acts in which the other person is involved or towards which he is directing his effort. Through processes too complex to be summarized here, Dr. Ehrenwald argues that the telepathic extension of the person into his interpersonal world is analogous to empathy. In fact, he seems to believe that one process passes over into the other, and that the process of enkinesis likewise passes over into a process by which we actually exert motor control over the world and of which psychokinesis would be a special case. The percipient in the telepathic situation is carrying out a process which lies a stage beyond empathy, while a successful subject in psychokinesis is at the same time expressing his need for extended control of the environment. Some cases seem to involve either telepathy, PK, or both, depending on one's point of view; when the object over which control is to be exerted is another person, we have a case of "the agent's tendency to enkinesis or vicarious functioning." (page 153) We have again a case of needs—not only on the part of one but on the part of two persons in the interchange.

The fact that the telepathic interchange is not simply an awareness by one person of something that he wants to know in regard to another, but often an active aspect of the attempt of one to help another is effectively brought out in the following example (page

181): "A German scholar living in the United States was preparing a historical study of Lenin's secret entry into Russia at the end of World War I. His article contained a reference to the famous Panzerzug. . . . He was at a loss to find the English equivalent for Panzerzug. Too tired to consult his dictionary, he decided to do so in the morning. . . . His wife had gone to bed a few hours before him. She had been unaware of the paper her husband had been writing. . . . First thing in the morning she reported the following dream: 'I heard an utterly ridiculous sentence in my dream. Someone said, "The lady is fair and armoured." Nothing else.'" This gave him the cue to his task.

Several chapters of the book relate to the way in which contemporary concepts in psychiatry about the interdependence of persons may throw light on paranormal processes. Take, for example, the fact that two mentally disturbed persons may each reflect the other person's problem and life situation, each one, so to speak, inducing disease in the other. Under some circumstances, moreover, two mentally disturbed persons may not be similar but, as Carington said, "countersimilar," that is, each one may have what the other lacks or may show, in an exaggerated form, what is defective in the other. Dr. Ehrenwald brings these contemporary psychiatric concepts across the line into the paranormal by suggesting that such "complementarity" may utilize psi processes as well as more familiar interpersonal processes.

If the reader is inclined to ask at this point what psychoanalysis has to offer except a renewed documentation of the thesis that unconscious needs show themselves through telepathy, the reply, I think, is that on the one hand psychoanalysis helps to understand the special *conditions* under which such needs are likely to do their work, offering a rich set of analytic hypotheses which can, in time, be tested, and that when once the telepathic factor has become evident, the analyst can work through the context in which it appears and use it for a fuller understanding, both of the psychopathology and of the general dynamics of psi.

For example, in the case of a patient who had severe anxiety about her mother on the very night on which the mother committed suicide, the case does not rest with the demonstration of the coincidence in time. It goes on to point out how the patient tried to attach

the dream to the fate of another person, her mother-in-law, rather than her mother; the way she phrased it was that something had happened to her mother "or maybe to her mother-in-law." She sought to deny its reality by going to the bank in the morning, before the bad news had come through, to parcel out the jewelry her mother was to have—a way of denying that her mother had died.

Some hints have already been given as to steps which might be taken, from this point forward, by the psychoanalytic investigator. Let us attempt to go further. First would be a systematic classification of all the paranormal phenomena which have been observed, rather than giving a few examples and saying that there were countless more. Second, more effort at establishing a control series would be of value to know from what population of events the chosen items are selected. A control series offered by Dr. Ehrenwald, for example, includes a number of patients who showed nothing which could be regarded as telepathic. It is only in this indirect way that we realize the enormous amount of material from which a few rather striking events have been culled. On reflection we all recognize that strange coincidences have happened under conditions where there is no reason at all to expect paranormal factors. It would be worth while to attempt some sort of control series in which there is no likelihood of a telepathic factor. It would be relatively simple, for example, to use some of the existing techniques to compare the dreams of certain patients with the events in the lives of psychiatrists who have nothing to do with the patient, or to compare the individual psychiatrist's experiences with the dreams of patients who are in no way connected with him. This would be laborious but it would be an important methodological step.

Third, it would be well worth while to attempt, say in the case of a single patient, a rather complete personality study in relation to all the telepathic or quasi-telepathic incidents that can be gleaned, whether they occur in the course of the psychoanalysis or not, in an attempt to get a closer definition of the life circumstances and character structure out of which psi predisposition comes.

Fourth, since telepathy from the analyst to the patient is taken for granted, it would appear not to be medically unethical to attempt—from time to time—definite emotionally charged experimental tests in which, so far as possible, things are constructive and important for

the patient, or deliberately aimed at him as a part of the therapy. As a matter of fact, this is not very far from the procedure of certain psychic healers, as for example Dr. Frederick Knowles, who has recently reported on such efforts and believes the outcome, in some cases, to have been paranormal.

In summary, this is a vividly and beautifully written book, making a plea for the recognition of the very wide operation of psi factors in the psychoanalytic situation. In the reviewer's judgment, Dr. Ehrenwald had already made his case strong at the time of his earlier book. The present book differs from the earlier one largely in the elaboration of theory. It is believed by the reviewer that the next step is not further complication of the theory but a delimited and well defined attempt at verification of the hypotheses offered.

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# GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which $n$ is the number of single trials, $p$ the probability of success per trial, and $q$ the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where $\sigma^2$ is the SD of a single run and $R_1$ and $R_2$ are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

falls to pieces when that job has been done and never quite "clicks" on another.

All of these are, as I have said, subtle and difficult problems. Their solution depends on the ability, the intuition, the adaptability, the imagination of relatively few people, possibly of only one person, in each organization. . . . And wherever you find a highly successful group I suggest you seek the causes for its success not in the organization chart, not in the budget book, not by counting uniforms or rank but by finding a man or a small group of men who have created the spirit of the place and who know how to preserve that spirit.

—L. A. Dubridge, "Science and National Security"
*Science,* December 31, 1954

## HERE AND THERE IN THE LITERATURE

Research success is the product of good ideas; and ideas cannot be manufactured like automobiles in a production line. Ideas arise in the brains of individuals, and they arise under circumstances that no one—not even the individual himself—understands. We *do* know how to increase the *probability* of new ideas arising. There are some simple rules for this: (i) Find some well-trained people who have been successful in getting ideas in the past. (ii) Give them full information about the nature and importance of the problem being tackled. (iii) Keep them in close touch with one another and with others engaged in similar work in a way to allow the maximum interchange of ideas, for out of such interchange and stimulation new ideas are frequently born. (iv) Provide these people with the facilities and help they need in developing and testing their ideas. (v) Keep the environment, the atmosphere, and the administrative arrangements such that there is the maximum stimulation to imaginative thought processes and the minimum of interruption and frustration.

Even these rules are not very specific. They do not tell how to find the right people, just how they are to be thrown together, what facilities they will need, or just what arrangements provide maximum stimulation and minimum frustration. All of these are delicate and subtle matters. They are also variable; a combination that works in one set of circumstances with one set of people may not work with others. Arrangements that are most satisfactory during an all-out war may prove hopelessly unsuitable in time of peace. An organization operating beautifully under one director sinks into mediocrity or worse under another. A research team that has delivered an outstanding contribution in the form of one new weapon

*(See inside back cover)*